SCIENCE

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LABORATORY ORGANIZATION[1]

PROBLEMS incident to a complete reorganization of a growing department, construction and equipment of a laboratory with adequate provision for a thousand students, and the laying out of suitable courses of instruction for a college proper, have so engrossed my time during the past two or three years, that I have had to forego in a large part the pleasure of the pursuit of research in my special field of inorganic chemistry and offering the results for your consideration. Such is the price the teacher, who loves his work, must and does pay. As the coming half year promises a consummation of the work in this direction to a fair degree and as it is the purpose of the authorities of the college to formally dedicate the new buildings in May next, to which ceremony it will be our pleasure in due time to request your attendance, it has appeared appropriate, in preparation for your visit, to present to you this evening and at the two succeeding meetings of the section, a series of three short papers under the titles "Laboratory Organization," "Principles of Laboratory Construction" and "Courses in Chemistry for Colleges." That such hackneyed titles offer little that is attractive is recognized, yet I am constrained to believe that what I shall have to say will serve as an incentive for discussion. In this connection allow me to remind you of the fact that the institution to which I shall refer more particu-

[1] Read before the New York Section of the American Chemical Society, February 7, 1908.

larly, belongs to you, to me, to every man, woman or child in this great city, who pays taxes or rent. I am merely one of the instruments for making it most effective. Having centralized my efforts, for the time at least, upon it, I shall welcome every suggestion by which the ideal may be more nearly approached. Human nature prompts a desire for helpful advice and sympathetic criticism. I may add, what is presented this evening will likely provoke an amused smile on the part of those of you who are associated with large well-organized corporations.

Every department of an educational institution requires some form of organization. A chemical department is more acutely affected by lack of organization than any other of the various subdivisions of an educational plant. The nature of the work classes a chemical laboratory under the head of department stores. Every chemical department has some style of organization. Much consideration of the subject has resulted in settling upon the plan here outlined as most suitable to an institution as the College of the City of New York, which is of collegiate grade only, not offering graduate or technical courses of study. Perhaps with slight variations parts of the plan may commend themselves to other institutions.

The pedagogic phase of the subject, first in importance, is to be considered as the last of the three topics. The material arrangements for securing the best results will be taken up next time. This evening I desire to direct your attention to the consideration of the executive phase, which involves the selection of a staff, the proper care for a large plant, its upkeep and control over current expenses, whereby the largest output and the greatest use may result with the least expenditure.

Complaint is frequently made in this country on the part of directors of laboratories and teachers of chemistry, that they are overcome with detail work. This complaint is, as a rule, well founded, but it is my belief that a system of organization can be adopted which will relieve the directors of much of the detail work, subdividing it in such a manner that it does not become too great a burden for any one individual.

CLERICAL HELP

In the first place it is uneconomical to require the head of the department to do much of the clerical work, which may be done by one who has not had to spend so much time in preparation for the duties of a directorship. In short, fuller return is made to the institution by the employment of a clerk or secretary, or both.

SELECTION OF A STAFF

As the head is held responsible for the department, his advice in the selection of subordinates should carry great weight. This is a principle generally recognized in all reputable institutions of learning, although not always lived up to. Many factors are involved in the selection of a staff. I shall limit myself in this connection to two pertinent remarks. Naturally we know our own graduates better than those who come from other institutions, and whom we have met incidentally. Our own graduates are more familiar with the local problems and it is easier to meet the difficulties with sympathetic help. In not a few institutions, however, in-breeding has resulted, if not in stagnation, at least in bringing about a state of affairs which has prevented the department from producing its best results. It is desirable, therefore, to hold enough of the men who know the inside life of the institution to preserve its traditions, yet new blood should be brought in continually to show the best of other

places and act as a stimulus for the whole. Withal it is better to have a man two years whom other people want, than to have a man twenty years whom nobody wants.

TIME FOR RESEARCH

It does not follow that the investigator is the best teacher, but a man of research at least has had a taste of the sweets of discovering the unknown, and consequently, with rare exceptions, is more capable of imparting some of that spirit to students than he who knows nothing of such experience. Research must be the prime means by which the young teacher can hope to attract such attention as may bring him preferment elsewhere. Yet he may not be wanted elsewhere, if he neglects the work at hand, namely, his teaching.

The controlling powers are often unfair to the young men who occupy the inferior positions in placing heavy teaching burdens upon them. Time for research, for by it advancement is hoped, is secured often only at the sacrifice of hours of needed rest and recreation, or by neglect of the little but important things of their teaching. I regard it really better business, if I may assume to speak from that point of view, to require less teaching hours for these young men, and to assign them certain times to be used for research, which may or may not be in cooperation with a colleague.[2] The freshness and vigor of his instruction will be improved and the responsibility for advancement will in a greater degree rest upon the man himself. A staff of ambitious young men secured under the same title and compensation gives an opportunity for competition for preference.

[2] We have provided one room where twelve tutors may have each a private place. Ten private laboratories are provided for those of higher grade.

STAFF ASSIGNMENTS

When a staff, or a nucleus of a staff, has been secured, it is advisable to organize the department into divisions. The extent of the subdivision and the duties assigned to the chief of each division will depend upon the number of men available and their qualifications. We have found it convenient so far to divide our department into divisions of general, analytical, organic and physical chemistry, and supplies. Each chief is held responsible not only for the instruction in his division, all details of equipment, etc., going first to him also, but for certain other matters, for example, the chief of the analytical division has general superintendence of the upkeep of the department; the chief of the physical division supervises the electrical equipment; the chief of the organic division oversees the museum and library, etc. A weekly conference of the whole staff gives an opportunity for suggestions and their discussion.

CONTROL OF NUMBERS

In a separate building given to any one subject in the curriculum of a college, there must be a fairly large assemblage of students at some time or other, particularly when a portion of the instruction is given by lectures to several hundred at once. Not only must the building be so constructed, but the organization so effected, that there may be rapid assembly and quick dispersion of people. Counter currents of the throng are avoided by having one stairway for going up and another for descending. Inevitable friction in the passages caused by discharging several groups of students into the corridors at once is avoided by a little foresight in arranging the schedule of recitations and laboratory practise. After three o'clock in the afternoon, after which hour no new class now begins, or in times of emergency, all means of exit

are used. They are so arranged that there is no converging of currents, the upstairs leading out one way, and the downstairs leading another. The exemplary conduct of the children in our public schools of New York at times of alarms of fire have often demonstrated the wisdom of such precautions.

STUDENTS' SUPPLIES

The problem of supplies is an ever vexing one with every chemical department. The College of the City is unique in requiring no fees, and by law we must provide sufficient apparatus and chemicals for each student to complete a first-class course in chemistry without cost to him. The cost for the average student, therefore, must be determined for each course. The student has that amount, in terms of supplies, to his credit upon which he may draft as his requirements arise. At the point of overdrawing on the part of an extravagant or careless student, his credit ceases and he must make good any excesses.

CARD CATALOGUE SYSTEM

Satisfactory bookkeeping is therefore a necessity. We have adopted the card catalogue system. The system for the division of supplies, including four auxiliary supply rooms, provides requisitions for immediate or temporary use, import orders, desk equipment and private laboratories; shipping instructions, inventory, labels, tags, etc.

A budget is prepared. The amount of the appropriation is known to the director. All requisitions are recorded with the estimated cost opposite in one column. When the bill for that particular requisition is presented the actual expenditure is placed in an adjoining column. By this means we live within our means, whether they be meager or extravagant. The Board of Estimate usually avoids the latter, even if we were inclined to extravagance.

The executive side of the system cares for students' registration, division registration, advancement, admission to advanced standing, record cards, requests for permit to work out of hours, notices of poor work and regular students' reports, etc. The cards are colored, which facilitates classification, as yellow for analytical, red for organic, etc. The selection of any designating color once decided upon is adhered to.[3]

In elaborating this system I have received hearty aid from my colleagues, Associate Professor H. R. Moody in particular.

PLACING RESPONSIBILITY ON THE STAFF

The use of a time stamp to indicate the receipt of invoices, date of approval of bills, for stamping of notebooks, etc., has proved of great value in our department in serving to place the responsibilities. In this connection it may be stated in anticipation of a subsequent discussion of certain pedagogic problems that we have the "section system" for laboratory instruction. We do not have large laboratories, but small ones, accommodating, as a rule, not more than twenty-five students at one time. The reasons for this will be discussed in a later communication. The instruction in each laboratory is given by one instructor. He is held absolutely responsible for the conduct of that room, including its physical condition. Instructors are human and when the responsibility is divided, one usually bears the brunt of the work and the filthy condition of a laboratory is always attributed to the "other fellow." This is a principle and one of fundamental importance. Its application

[3] Samples of the cards in use were exhibited. They may be had on request to the author.

discloses the slipshod clock-watcher, while it rewards the earnest and worthy.

UNIFORM REAGENTS

To bring about uniformity in the use of chemicals throughout the laboratory, where it is possible, reagents are made up in bulk according to a standard, which has a normal basis. These reagents, on requisition to the main stores, are distributed in proper vessels and charged to the respective laboratories to which they are issued. As the instruction in one laboratory is as a rule limited to a particular kind, and as several laboratories are carrying on the same kind of instruction, it is comparatively easy at the end of the semester to determine the consideration given to economy by the instructor in charge. Parsimony may not be a desirable quality in a man, but economy is not a bad habit.

REAGENT BOTTLES

The missing reagent bottle has been the source of unending worry to instructors and the cause for much useless delay to the student in his work. When the number of reagent bottles goes beyond ten thousand, the problem is serious and the exasperation resulting is likely to be beyond words. It may be solved in large part by burning numbers indicating the floor, room and desk in with the label. The number of the stopper and bottle is cut by a diamond or etched. A glance shows the completeness of a set of reagents or the location of a misplaced bottle, for bottles will be misplaced as long as human beings use laboratories. There is only one system which will prevent that, and it is not allowed by the law. I mean the shotgun system.

LOCK SYSTEM

In order to hold a student responsible for the apparatus with which he is charged, he must be provided with locked cupboards and drawers. These in some laboratories have combination locks. They get out of order and are wasteful of the instructor's time in the semestral cleaning up and refitting. Therefore, we adopted the separate key system, the instructor being supplied with a master-key. To avoid the frequent excuse of leaving keys at home, they are kept upon numbered hooks within a cupboard, provided with a clear glass front, next the bulletin board, which is conveniently placed in each laboratory by the entrance door. The cupboard is opened only by the instructor's master-key. A glance indicates absences and prevents the use of the laboratory by students out of hours without a special permit.

The key system adopted for the entire department may be of interest. The stock-room system (six rooms) are under one key. Each stock-keeper has one. All students' laboratories, lecture and quiz rooms, the key cupboards, students' desks, toilets, janitor's closets, switch boxes for lighting, library and doors to the building are opened by one master key. Each member of the staff has one of these. The toilets, janitor's closets, and laboratories are under one key for the cleaners. The suite of executive rooms, including the private laboratory of the director, is under one key. The director's secretary, private and lecture assistants have one. Each private laboratory is under a separate key, that of the chief of the division of physical chemistry controlling the switchboard, storage battery and electric furnace rooms. The director is provided with a grand master key which opens every lock in the building except the private desk of each member of the staff.

ELECTRIC CURRENT SYSTEM

In pursuing the pedagogic policy to which reference has been made, it is neces-

sary to provide students with electric current from storage batteries. These delicate sources of energy require systematic and intelligent supervision. The instructor in charge of a particular laboratory makes requisition upon the chief of the division of physical chemistry for current of definite voltage and an outside limit of amperage. The cells are connected with plugs carrying fuses with the limit of amperage wanted. A careful record of the condition of the cells, when put in service and when disconnected, is made. Communication among the offices, private laboratories (for a staff of thirty), stock rooms, preparation rooms, etc., is facilitated by an intercommunicating telephone system, whereby eight different conversations may be carried on simultaneously and without interference.

SEMI-ANNUAL CLEANING UP

The semi-annual cleaning of movable ironware like files, tripods, etc., may best be accomplished by dumping the pieces into an electrolytic tank containing water to which salt has been added. After the passage of the current for a few minutes, the cleaned metal is washed with tapwater and placed in a drying oven. Those articles requiring painting are then dipped in a tank of acid-proof paint and allowed to drain on an incline which leads the excess of paint back to the tank.

CONTROL OF PLATINUM

We have found it advisable to mark all our platinum ware by a special stamp and register it with each dealer in platinum with the request that any of that metal bearing the "Sign" presented for sale without an accompanying letter of the director, be held pending communication with the department. We have met with gratifying cooperation on the part of the dealers.

FIRE PRECAUTIONS

It goes without saying that precautionary measures must be taken against the inevitable fires often recurring in laboratories. In each laboratory, depending upon the size of the room, we decided to place tube powder fire extinguishers. The larger rooms have a tube at each end. In the corridors on each floor and in the organic laboratory is an improved Babcock machine. That failing, recourse is had to the fire hose conveniently placed. Each laboratory is connected directly by enunciator bells with the office of the assistant to the director, who is chief of the division of supplies and *ex-officio* chief of our voluntary fire brigade.

In the event that an accident has resulted in setting fire to an individual, recourse is had to the shower which extends over the exit door of each laboratory for students, and the fire-proof blanket hung nearby. Emergency medical closets are in the corridors of each floor. An accurate detailed statement of each accident, however small, is filed with the director by the responsible instructor within twenty-four hours. These records are kept open for court or medical inspection.

By the system outlined the head professor secures some time for service on many important committees involving general problems concerning the welfare of the institution. The division of labor has not resulted in any complaints about having placed too heavy a burden upon any one of my associates. Each member of the staff not only earns his salary legitimately, but secures some hours for investigation. The city gets a constant rich return for the investment, for the service is not only good, but fuller, when the work of the individual is stimulated by a happy ambition.

Charles Baskerville
College of the City of New York

THE CARNEGIE FOUNDATION NOT A CHARITY BUT AN EDUCATIONAL AGENCY[1]

THE year and a half of experience in the administration of Mr. Carnegie's great gift has served to reveal not only some of the results likely to accrue from it, but also some of the tendencies in administration which are to be avoided.

The gift was intended to serve primarily in the establishment of retiring allowances for teachers in the higher institutions of learning in the United States, Canada and Newfoundland, but the donor left it to be administered for this purpose in such manner and under such regulations as the trustees might decide to be wise.

The fears which have been expressed in certain publications that a great gift like this in the hands of a limited number of men might prove a centralized power which would hinder rather than aid the progress of education, do not seem to me well founded. The trustees of this foundation are in the main college and university presidents who have come up through the profession of the teacher and who are not likely to lose touch with the needs and aspirations of teachers. Furthermore, they compose a board which while continental in the interests represented has no constituency to cultivate either for the sake of numbers or of revenue. If the board gain influence it can come only through a just and wise administration of its trust. It is, in my judgment, a wholesome influence in education to have a few such centralizing influences. Our tendencies in the past in the founding and maintenance of colleges have been almost wholly along competitive lines. Colleges and universities have grown up not only without any effective outside criticism, but without any conscious attempt to serve the larger interests of education, letters and science or to operate on a national scale. Here for the first time is created an agency which is conscientiously seeking to consider the problems of institutions from the larger view of the welfare of the teachers in all colleges and universities, and to take into account the interests not alone of a community or of a section, but of a continent. The two viewpoints are vastly different. Heretofore the tendencies have nearly all been centripetal and the outcome is seen in the multitude of weak, badly organized, and in some cases unnecessary institutions. The establishment of an agency which is concerned with the larger outlook and the wider field can scarcely fail to make for educational coherence and in the end for educational unity.

There lies also in the work of such an agency increased possibilities for international understanding and betterment. Canada and the United States can each learn from the other in the matter of education. The common school systems of the two countries are remarkably similar in their organization and in their methods. The Canadian institutions have naturally followed more closely than American colleges English precedents. Just at this time, however, the American college is undergoing a searching examination and methods are again being developed which look toward the English college ideals and organizations. Each country will gain by an acquaintance with the educational methods of its neighbor and such acquaintance makes for improved international relations.

The year and a half of experience in the administration of the foundation has served to make clear at least one principle, namely, that the retiring allowance must come as a right, not as a charity; as a thing earned in the regular course of service, not a

[1] From the second annual report of the president of the Carnegie Foundation for the Advancement of Teaching.

courtesy. The establishment of a retiring allowance system upon definite rules under which a professor receives his retired pay through his college in due course can not fail to strengthen the teacher's profession enormously. The administration of this fund as a charity would in the long run be equally sure to harm rather than help the teacher and the cause of education.

For the demoralizing effect of a pension system supposedly administered on the ground of personal merit one has only to look at the history of government pensions in this country. These pensions were originally instituted to dignify and assist men who had served the country honorably and unselfishly. Their administration has become the greatest single source of political corruption of which our national government has been guilty, a contagion which has touched presidents, members of congress and, most of all, the class intended to be benefited. Human nature in teachers is not materially different from human nature in congressmen and soldiers. No body of men is wise enough to administer a system of pensions upon considerations of individual merit only, without a strong probability that the administration will in the end degenerate.

The preconceived ideas in the minds of the public and in the minds of many teachers concerning the work of this board contemplated a very simple task. On the face of it the allotment of pensions to worn-out but deserving teachers who might apply directly or through their friends did not seem to involve any very difficult problems. Such a picture of benevolence appeals to much that is best in our human nature. But such an administration of the funds of this trust would be comparable in its ethical results to that which might be had by standing on a street corner and giving a gold piece to any aged passerby who seemed on the whole to merit it and to need it.

There are many deserving men who have grown old in teaching who assumed that the funds of this board would be distributed in some such way and who have felt disappointed that they have not been so distributed. A considerable proportion of these teachers have done their work in schools below the college, many are in denominational institutions and are thereby excluded, and very few of the remainder have rendered to education such a service as would justify an exception in their cases. The very making of exceptions is itself to be regretted. At the beginning of its work the board of trustees of the foundation decided that it was desirable, as far as possible, to confer retiring allowances through the institutions themselves, but that it was just and fair to extend the benefits of the retiring allowance system to a number of individual teachers, particularly to men who had done pioneer work in education or who had rendered extraordinary and unusual service in its cause. This policy has thus far been continued, and I believe without unfavorable results, although experience shows that the number of teachers who are believed by their friends and acquaintances to have rendered extraordinary and unusual service is practically without limit. In fact, every region and almost every institution has its representative who in the opinion of those near him is deserving of special consideration. To provide pensions for certain of these deserving teachers and to refuse them to others will not work for the betterment of the teacher's calling, the improvement of colleges, or the progress of education. In a word, the awarding of the retiring allowance in any other way than through institutions and in compliance with fixed rules under which the retiring pay comes as a right,

not as a favor, is at best a process of doubtful value. Within a limited time it will, in my judgment, be necessary to grant retiring allowances only through institutions.

The true task of this board is not to pass upon the merits of individuals, but of colleges; to decide upon such educational standards as seem fair and wise, and then to proceed to admit to the system of retiring allowances such institutions as, complying with these standards, come within the provisions of the charter and the deed of gift. To do this involves a study of the educational situation in the United States, Canada and Newfoundland. The first step toward such a study is the bringing together of the facts themselves concerning these institutions, such as their method of government, their denominational relations, the value of each institution as a center of intellectual and moral influence, their financial resources, and, most important of all, their academic standards of work. In a word, the Carnegie Foundation for the Advancement of Teaching must be first an educational agency before it can act wisely in awarding retiring allowances. It is charged with the duty of administering a fund for higher education, for teachers in colleges, universities and technical schools of college grade. Its first concern is to ascertain how many such institutions there are and which of them are entitled on fair and reasonable conditions to the privilege of this fund.

SCIENTIFIC BOOKS

Electro-analysis. By EDGAR F. SMITH, Professor of Chemistry in the University of Pennsylvania. Fourth edition, revised and enlarged, with forty-two illustrations. Pp. 336. Philadelphia, P. Blakiston's Son and Co. 1907.

The introduction of modern electrolytic methods into chemical analysis has brought about in that science a veritable revolution, in which the author of this compendious but handy volume has been a prominent leader. It is with authority that he discusses the various electro-analytical methods of which several hundred are described in the text, for many of these methods were devised and apparently nearly all have been tested in the author's laboratory.

The book is quite frankly a collection of receipts which if followed to the letter will lead to successful analyses. When a deviation from the prescribed rule will lead to disaster, or why this method succeeds and that method fails, the reader is not told. Nor is this altogether the fault of the author. Most of the methods here given are like cook-book receipts, based on pure empiricism. We do not know why the investigator tried a given method. We only know that he tried it and it "worked." In general the practise of quantitative analysis has so far outstripped the theory that at present it may be considered more an art than a science. This state of things the author accepts without protest. A chapter entitled "Theoretical Considerations" is devoted to Freudenberg's work on the separation of metals at constant E.M.F. Aside from this no mention of theory is made except occasionally when one of the old-fashioned theories is treated as an experimental fact, for example, on page 111 it is stated as if it were well established that in the electrolysis of potassium oxalate, potassium deposits on the cathode and later reacts with the water to produce hydrogen and alkali.

The directions given in the book are as a rule clear and explicit, but not always. Thus on page 109 we read, "add 4 c.c. of a solution of ammonium acetate, 20 c.c. of citric acid, and dilute to 200 c.c. with water." It would be convenient to know the strength of the ammonium acetate and citric acid solutions which are to be added. However, such omissions are infrequent. A more serious criticism may be made which applies to every process described in the book, and it is not that the author gives too few directions, but too many to be mutually compatible. On nearly every page we are told to electrolyze

with, say, a current density of 5 amperes per 100 sq. cm. and an E.M.F. of 5.5 volts. Now when a cell is constructed of given dimensions, and filled with a solution at a certain concentration and temperature, the voltage required to produce a given current density is fixed by the conditions. It is true that the potential fall at the electrode is a very important factor in metal separations but this fall is determined directly by the current density. The voltage at the cell terminals is of no consequence in the analysis. From the directions for each process the prescribed voltage given by the author may be entirely omitted and must be in order to make the directions self-consistent.

In discussing the various processes the probable experimental error is not discussed, but fortunately figures are often given showing the results of actual analyses and from these the probable accuracy of the different methods under favorable conditions may be estimated.

Especial attention is given to the deposition of metals on a mercury cathode and to the use of rotating electrodes. The latter device, which permits an analysis formerly requiring several hours to be carried on now in a few minutes, was invented by v. Klobukow in 1886 but has only been generally adopted within the last few years. Its efficiency is apparently due merely to the relative motion of electrode and electrolyte which prevents the depletion of the electrolyte in the neighborhood of the electrode and also perhaps by mild attrition produces a more coherent film of metal. It is therefore possible to use a very high current density without the formation of loose or poorly adhesive films.

In the chapter on the history of electroanalysis it is interesting to learn that even in 1801 it was proposed to separate the metals by electrolysis, and that in 1860 Lucknow deposited the metals quantitatively and separated them from one another by means of the current, and even determined lead and manganese by depositing them as peroxides at the anode.

Besides chapters on the determination and separation of metals there are others on the determination of halogens and nitric acid, on electrolytic oxidation and on combustion of organic compounds by electrical heating. There is an index, and each chapter is introduced by a short bibliography. The volume is of a convenient size, is bound in flexible covers and is an excellent specimen of bookmaking.

Gilbert N. Lewis
Massachusetts Institute of Technology

SCIENTIFIC JOURNALS AND ARTICLES

The American Museum Journal for April contains a well-illustrated article by Barnum Brown on "The Trachodon Group" of two mounted skeletons of these strange dinosaurs. There is a plate of the large African elephant head, obtained by the Tader expedition and mounted by Mr. Lang. Under "Ethnological Loan Collections" is a description of the exhibit illustrating the life of the Ojibwa Indians deposited in a branch of the public library where it is seen by children to whom the story of Hiawatha is told during the "story hour." Recent museum publications are noticed and announcements made of the various lecture courses.

The Bulletin of the Charleston Museum for March under "Museum Records" notes the progress in caring for the collections and tracing the history and records of material that has been accumulating for more than a century and includes "the fossil man of Guadeloupe," the Elliott Herbarium and specimens noted by Holbrook, and Audubon and Bachman.

The Evolution of the Elephant, by Richard S. Lull, forming Guide No. 2 of the Yale University Museum comprises 44 pages, with many illustrations and four maps of distribution of fossil and living forms. It includes a general discussion of the characters and affinities of the Proboscidea, the evolutionary sequence of the genera and species and the indicated migrations of the Proboscidea which led to their present distribution.

The Museums Journal of Great Britain for March has an article on "Museums of Elementary and Higher Grade Schools," by

Herbert Bolton, pointing out their usual mixed character and lack of relation to the school curriculum and suggesting how they may be made of real value. G. A. Dunlop describes "Drying Plants without Pressure" by the use of fine sand or boxwood sawdust, the latter material preserving many of the natural colors and much of the texture of flowers and leaves. It is pointed out that care should be taken in using sawdust owing to its irritating effect on the mucous membrane of the eyes and air passages. There is an account of the exhibit of "British Grown Trees in the Natural History Museum." Among the many "Notes" is one to the effect that the London and North-Western Railway is gathering material to be ultimately displayed in a railway museum.

The Museum News of the Brooklyn Institute for April contains articles on "The Giant Stork, or Jabiru," "Zuñi Silver Working," and "Lizards in Human Stomachs." The section devoted to the Children's Museum discusses "Elementary Physics as a Subject for Children's Museum Instruction" and notes the various living animals at the museum.

A NEW psychological journal, *Le revue psychologique,* has been established at Brussels under the editorship of Mlle. I. Ioteyko, M.D.

SOCIETIES AND ACADEMIES

THE NATIONAL ACADEMY OF SCIENCES

THE scientific program of the meeting of the National Academy of Sciences held in Washington on April 21, 22 and 23 was as follows:

"A Proposed International Atlas of Land Forms," by W. M. Davis.

"The Geological Age of the Santa Cruz Beds of Patagonia with Restorations of Santa Cruz Mammals" (with lantern illustrations), by W. B. Scott.

"The Biological Station for Research, at Agar's Island, Bermuda" (with lantern illustrations), by E. L. Mark.

"The Cytological Basis of Heredity and the Determination of Sex" (with lantern illustrations), by E. B. Wilson.

"On the Functions of the Parathyroid Glands in their Relation to Calcium Metabolism and to Tetany," by W. G. MacCallum and C. Voegtlin (introduced by W. H. Welch).

"Supplementary Atmospheres," by T. C. Chamberlin.

"Great Tangential Movements of the Earth's Crust," by Bailey Willis (introduced by Ira Remsen).

"Some Results of the Magnetic Survey of the United States," by L. A. Bauer (introduced by R. S. Woodward).

"The Metasilicates of Lime and Magnesia—An Application of Physical Chemistry to Minerals," by E. T. Allen (introduced by R. S. Woodward).

"The Exact Measurements of Quantities of Heat, up to 1,500° Centigrade," by W. P. White (introduced by R. S. Woodward).

"Spermatogenesis in the Bee and in the Wasp," by E. L. Mark and Manton Copeland.

"Biographical Memoir of Alpheus Hyatt," by W. K. Brooks.

"Perceptions, Ideas and Hallucinations," by J. McK. Cattell.

"Application of Periodic Solutions of the Problem of Three Bodies to the Motion of the Moon," by F. R. Moulton (introduced by E. H. Moore).

"Biographical Memoir of Asaph Hall," by G. W. Hill.

"The Elevated Reefs of Mombasa and Adjacent Coast," by A. Agassiz.

"The Pelagic Fauna of Victoria Nyanza," by A. Agassiz.

"Recent Work of the Smithsonian Astrophysical Observatory," by C. G. Abbot (introduced by Chas. D. Walcott).

"The Hydration of Ions in Solution," by E. W. Washburn (presented by A. A. Noyes).

"Radiant in the Star-group in Taurus" (with lantern illustrations), by Lewis Boss.

THE BIOLOGICAL SOCIETY OF WASHINGTON

THE 442d meeting was held on March 21, 1908, with President Stejneger in the chair.

The first paper was by Professor W. J. Spillman, on "Fixation of Breed Characters in Animals and Plants." He first illustrated the manner of behavior of a character pair (non-albinism and albinism). The character of the population from the second to the tenth generation of the descendants of a Mendelian hybrid was illustrated by lantern slides for different methods of selection. First, with no selection, it was shown that with close

fertilization the descendants of a hybrid constitute, after a few generations, a population consisting of 2, 4, 8 and so on, fixed types, each constituting half, a fourth, an eighth and so on, of the population, depending on the number of character pairs considered, the number of the fixed types resulting being equal to two with an exponent equal to the number of character pairs. Along with these fixed types is found a decreasing proportion of hybrids between the fixed types. With promiscuous cross fertilization and with no selection to type the proportion of the various types occurring in the second generation of the hybrid remains the same in later generations.

By that method of selection in which extracted recessives are discarded it was shown that both with cross fertilization and close fertilization the population gradually approaches the type selected. In close-fertilized plants the approach is rapid, making in ten generations a nearly pure population. With promiscuous cross fertilization this method of selection tends to produce a pure population, but at a much slower rate than with close fertilization. For instance, with two character pairs, when cross fertilization occurs, in the tenth generation the type selected becomes approximately 65 per cent. of the population, while under similar conditions, with close fertilization, the type selected becomes approximately 99 per cent.

With animals another type of selection is practicable, viz., that in which one of the sexes is tested for gametic purity and only homozygotes of the type desired used. When this method is used the selected type increases even more rapidly than with close fertilization and elimination of extracted recessives.

In plants still a different process is practicable, viz., that of testing each second generation individual by saving its seed and planting them separately. When the next crop matures the character of the progeny of the various individuals enables the breeder to select those which are homozygote and those thus establish at once a pure breed.

The speaker also referred to the so-called elementary species secured by Nilsson, of Sweden, and Shull, of this country, explaining them as the descendant of a completely homozygote individual. It is claimed that in these completely homozygous forms all hereditary variation is completely eliminated and that only such variation occurs as is due to environment.

Dr. C. O. Townsend asked whether the speaker considered variation caused by environment to be hereditary. In answer it was stated that that question had not been sufficiently investigated to permit of a categorical answer. When plants are moved to an entirely new environment, important variations frequently occur. Some of these variations are hereditary, at least under the new environment. Whether they would be permanent should the descendants of the plant be returned to the original environment has not been sufficiently investigated.

Mr. M. C. Marsh read the second paper, describing "A New Method of Marking Salmon Fry." Distinctive marks on young salmon, provided they do not interfere with the life and movements of the fish and are recognizable in the adult, are very useful in acquiring information concerning the age and migrations of salmon and the efficiency of artificial propagation. The Bureau of Fisheries has obtained definite returns from salmon fry marked in Alaska by the amputation of certain fins with scissors. These marks were placed upon fry about three months old. The operation of marking is rather slow and tedious. The possibility of rapidly marking salmon fry in the sac stage or at the time of planting, by cauterizing the fins with a thermo-cautery needle was shown. Either the adipose, dorsal or anal may be removed in this way and probably other fins. The shock of the operation causes a moderate loss which ceases after two or three days and the remaining fry recover. Evidence thus far gives reasonable assurance that regeneration will not interfere with the recognition of the marked fish which return as adults. Combination marks on the same fish may to some extent be made and in this way the number of distinctive marks may be increased. A given

mark should, of course, be confined to one locality and one season, unless an interval of years has elapsed.

A character shaped like the letter s may be branded upon the skin of salmon fry by the aid of a thermo-cautery with small mortality. The subsequent obliteration of the wound by healing and regeneration, and failure of the scar to grow sufficiently with the growth of the fish, are possibilities which may interfere with recognition of the adults. Salmon fry, however, are to be marked by these methods on a considerable scale in Alaska, and the outcome awaited.

By partly stupefying the salmon fry with chloroform the marking and branding is greatly facilitated and the loss reduced.

A method for marking the fry of the great commercial species, such as the cod, shad and whitefish, is greatly desired. Such fry are too small and delicate to withstand the shock and handling incident to a mechanical mark.

The last paper was by Dr. M. W. Lyon, Jr., consisting of "Remarks on the Horns and the Systematic Position of the American Antelope." The history of the literature on the subject of the periodic shedding of the horns of the American antelope was reviewed and an outline of the growth of the horns and the method of shedding presented. Two abnormal cases in which horns were not annually shed were mentioned. Dr. Lyon concluded that in structure and method of growth the horns of the American antelope do not differ essentially from the horns of the cattle, sheep, goats and true antelopes, family Bovidæ, and that the genus *Antilocapra* should not rank as the representative of the family Antilocapridæ of the order Artiodactyla, but as a subfamily, Antilocaprinæ, of the family Bovidæ. Illustrations of the growing horns of males, of female horns, and of an abnormal horn which had not been cast off were shown.

M. C. MARSH,
Recording Secretary

THE ANTHROPOLOGICAL SOCIETY OF WASHINGTON

AT the meeting of March 3, 1908, a necklace, consisting of a human lower jaw suspended from a string of beads and human teeth, and two leather bands ornamented with ends of fingers, all from the Apache and Ute Indians of the southwest United States, were exhibited and commented on by the president and secretary, who referred to the use of parts of the human body for fetichistic purposes. The paper of the evening was by George R. Stetson, "The Industrial Classes as Factors in Racial Development." Mr. Stetson said that the factory began in England under the Romans and that early the evil effects of overcrowding, bad air and lack of sanitation were known, causing attempts to be made to alleviate the troubles. In England, in 1906, 109,068 factories employed four and one fifth million workers, seventy-one per cent. women and children, and in Scotland sixty-eight per cent. were females. Mr. Stetson named various occupations which are dangerous, laundries being the worst. Cotton mills are damp and sometimes have 31–49 volumes of carbonic acid to 1,000 volumes of air, whereas, the law allows only nine volumes to 10,000 volumes of air. In fact few mills are sanitary and the result is to lower the weight, stature and general health among children, and produce rachitis and trade diseases. Much of the terrible deterioration shown among recruits offered is due to the perils of the modern manufactory. Overcrowding, also, is a great evil, which causes pauperism and disease. In Sheffield, for instance, the infant death rate is 236 to 1,000, and in other localities the condition is deplorable. Mr. Stetson said that deterioration is more marked than it was twenty-eight years ago, and it is manifest that the industrial classes must be relieved by some means or the viability of the race will be decreased. The paper was discussed by Professor Holmes, who said that in a small way much has been done to protect the worker, but to secure ideal conditions is difficult and that there would probably always be a certain amount of deterioration; by Dr. Hrdlicka, who said that the employment of children is the chief source of danger, because the seeds of disease are implanted at a period when the body is undeveloped; by Major Clark, who

said that child labor is not an unmixed evil, and that it is chiefly a sanitary question; and by Mr. Robinson, who suggested old age pensions as means of avoiding pauperism.

WALTER HOUGH,
General Secretary

THE GEOLOGICAL SOCIETY OF WASHINGTON

AT the 202d meeting of the society, held at the Cosmos Club, on Wednesday evening, March 11, 1908, under informal communications, Mr. W. H. Emmons presented a formula for determining the angle of intersection of a plane with the strike of a vertical section which is drawn oblique to the strike of the plane.

Regular Program

Preliminary Notes on Recent Earth Movements at Butte, Montana, as shown by Precise Levels of the U. S. Geological Survey: R. H. CHAPMAN.

In 1904 precise levels were run at Butte, Montana, and many bench marks established with the purpose that they should be used as a base to compare future changes of surface level.

In 1906 the Coast and Geodetic Survey brought one of its precise lines into Butte and obtained checks on several benches of the 1904 work. These checks show changes which are interesting. Using a bench mark near the Butte Reduction Works as a datum and extending the comparison of benches northward an increasing difference is found.

	Difference
Butte Reduction Works	0.000
Webster School	— .017
Colorado Street School	— .051
Owsley Block	— .436
Court House	— .737
Government Building	— .888

From the Government Building a comparison along another line to the flat southward shows a decreasing difference.

	Difference
Government Building	— .888
Florence Hotel	— .791
Braund House	— .130
South Butte	— .051

Butte Reduction Works again the zero.

The maximum change at the Government Building represents a sinking of .444 foot per annum.

The Owsley Block compared with the Butte Reduction Works shows a sinking of .344 foot between 1899 and 1904, and of .436 foot between 1904 and 1906, the former giving a rate of .069 and the latter of .218 foot per annum. The figures obtained from the earlier work are not so reliable as those depending upon the notes of 1904 and 1906, but comparison shows the rate of change to be increasing.

The bench marks were located in the hope that eventually it would be possible to determine if there is continued movement along the local fault planes, but the 1906 work did not include a sufficient number of checks to be of value for this purpose.

No reliable comparison has yet been had with the benches established on the east side of the continental fault of the Rocky Mountains.

The systematic repetition of check levels at intervals of two years—to bring out the facts of value—was strongly urged.

Pleistocene Phenomena of Central Massachusetts: WM. C. ALDEN.

The Worcester County plateau is well glaciated, retaining in but few places residuum of preglacial weathering. There is much drift in the valleys, but on slopes and crests it is very thin, probably not averaging 15 feet over the area.

No undoubted evidence of pre-Wisconsin glaciation was observed. Drumlins are numerous, while in places the bed-rock surface between them is but thinly covered. Consideration of the elevation of the glaciated summits of Mounts Wachusett and Monadnock, their distances from the terminal moraine, and other factors leads to the inference that there may have been 1,500 to 1,900 feet of ice over the central part of the area, and 2,000 to 3,000 feet in the Connecticut Valley.

No moraines of recession are traceable continuously across the area. Terminal moraines, kame and outwash terraces, eskers and delta plains and deposits formed in temporary glacial lakes are numerous, but while they enable

one to determine fairly well the positions of the retreating ice front at these particular places, the deposits are almost wholly confined to the valleys, not being traceable across the intervening ridges. However, a consideration of all the factors affords a basis for their correlation as marking thirteen stages of halt in the recession of the ice front across the 30-minute quadrangle. These seem to show a probable gradual development of a broad re-entrant over the Worcester County plateau with lobes lagging behind in the Connecticut Valley and on the lower ground to the eastward, so that in the western part the retreat was from southeast to northwest, and in the eastern part from southwest to northeast.

The phenomena noted above appear to have been formed immediately in connection with the retreating ice front. Nothing was observed indicating that the elevation was lower than at present or that there was an incursion of the sea up the valleys following the deglaciation.

Notes on Recent Changes in the Bogoslof Islands: PHILIP S. SMITH.

In 1900 Merriam summarized the changes that have taken place in the Bogoslof Islands up to the time of the Harriman Expedition of 1899. Since that time two new volcanoes have been formed and one of these later peaks has disappeared. Many of the stages in the rapid changes noted in the islands have been mapped by different observers, especially in the critical years 1906 and 1907. Maps made by Dunn and Stromburg in 1906 were exhibited, but the most interesting series were maps made by Camden, July 4, 1907, Jaggar, August 7, 1907, Camden, August 11, 1907, and October 16, 1907. The two last maps represented the conditions approximately a month before and a month after the disappearance of McCullock Peak, the new volcano which was formed between September, 1906, and July, 1907. Fragments of the ejectementa collected by the Revenue Cutter Service, on examination showed at least two distinct kinds of rock; one an andesite with both hornblende and augite, a rather basic feldspar and an abundance of volcanic glass; the other showed a granular rock similar to a diorite with essentially the same minerals as those of the andesite, but crystallizing with a distinct hypidiomorphic texture. None of the specimens were collected from ledges and it is not known whether or not the diorite is from a fragment riven from the walls of the conduit and thus brought to the surface. The presence of granitoid rocks on the southern margin of the Aleutian chain, however, suggests that the basement on which the recent volcanics have been poured out is in many places a coarsely crystalline granular plutonic rock. No flows of lava were noted in connection with any of the recent eruptions on the Bogoslof islands.

PHILIP S. SMITH,
Secretary

THE PHILOSOPHICAL SOCIETY OF WASHINGTON

THE 649th meeting was held on April 11, 1908, President Bauer presiding. Mr. Bailey Willis, of the U. S. Geological Survey, presented by invitation a very interesting paper on "The Mobility of the Lithosphere."

The address of the last retiring president of the society, Mr. John F. Hayford, was on "The Earth a Failing Structure." In it Mr. Hayford discussed the relations of weight, mass and stability of the earth, and gave physical and geological evidence in favor of the view that the earth yields more or less constantly to the internal stresses due to gravitation. Mr. Willis took up this theme along the lines of geological evidence and presented the conclusion that no considerable mass of the lithosphere has been in a state of rest, that is of static equilibrium, within any period known to geological history.

Referring to the distinction initially made by Powell and Gilbert between epeirogenic and orogenic movements in the earth's crust, the speaker first showed a series of lantern slides illustrating different geographic conditions of North America from late pre-Cambrian to the present time. These charts are necessarily, in the present state of the science of paleogeography, of a provisional nature, and subject to material modification, so far

as details of distribution of land and sea at any particular epoch are concerned; but there is no doubt as to the general character of the changes which have occurred from period to period, and these changes are of very notable degree. It was shown that we could distinguish at least four great diastrophic cycles, each one of which consisted of an initial epoch of pronounced continental emergence associated with mountain building, and a later period of planation of lands to a peneplain condition and very wide marine transgression. Of these four cycles, the latest is that in which we live, and we are at the present time in the initial epoch of that cycle which is marked by decided mountain features. If it be assumed that the volume of the oceanic waters has not varied materially throughout the geologic ages which these cycles comprise (and the physical evidence substantiates this assumption) it follows that changes in the superficial extent of seas and lands must be the result of changes in the shape and depth of oceanic basins; but such variations of form can only proceed from movements of the lithosphere, movements which have the character of warping; and as such variations are traced throughout the entire legible period of geologic history, it follows that movement has been an equally persistent factor. From the nature of the effects, which as stated in the early part of the cycle are more vigorous and in the later part more gradual, it follows that there is a pronounced variation and a certain periodicity in the movements; but we are not able to distinguish any time when epeirogenic change has not been in progress in some perceptible degree.

Taking up the other aspect of diastrophism, namely, orogenic or mountain-building movements, the speaker showed a map of the world on which were delineated the axial trends of great folded mountain chains. From a study of their distribution and of their relations to areas of maximum elevation and denudation it appears (*a*) that the Atlantic and Pacific oceanic basins have widened at the expense of the continental margins which have been crushed in; and (*b*) that the whole southern hemisphere (the oceanic hemisphere) has spread northward at the expense of the northern (continental) hemisphere. The very extensive areas about the Arctic Ocean, which have remained undisturbed since an early Paleozoic time, have formed a central mass against which the more southern portions of Eurasia and North America have been folded.

From the detailed consideration of these orogenic movements Mr. Willis concludes that the great tangential displacements are due to an expansion of the suboceanic masses. This conclusion is independent of any particular hypothesis of the causes of such expansion, but in his opinion the effects are so distributed as to be inconsistent with any interpretation on the lines of the contraction theory. Following Dutton, Gilbert and Hayford in the general concept of isostatic adjustment toward an equilibrium among denser and lighter masses of the lithosphere, he holds that the persistent epeirogenic movements as well as the occasional orogenic movements are due to strains set up among the heterogeneous bodies of the earth's crust which differ from one another in density. It appears obvious that were the suboceanic masses enough denser than the subcontinental masses, the former would displace the latter, provided the firmness of rocks be insufficient to maintain their form. In masses of large dimensions, cubes measured by tens of miles on the side, rocks are not firm enough, even under the pressures that exist down to a depth of 100 miles below the surface, to maintain their form if unsupported at the sides; and thus there must exist throughout the lithosphere a tangential strain or tendency to spread which is somewhat greater in the denser than in the lighter masses. This strain is apparently not initially sufficient to cause movement if we may assume that the lithosphere were at any particular instant in a state of equilibrium; but it is a persistent strain of constant direction in any particular locality, and in the event of disturbance through changes of molecular arrangement or temperature, it would serve to direct the effects of any growing strain and ultimately to cause movement in the effective

direction, that is from the denser towards the lighter mass.

Mr. Willis thus explains the persistency and periodicity of movement in the lithosphere through the efforts of unstable heterogeneous masses toward equilibrium; efforts which are assisted by variations in molecular arrangement, temperature and pressure, and which are directed by the tendency of the heavier masses to spread at the expense of the lighter masses. The resulting tangential strain grows during a prolonged period and produces more or less incomplete efforts towards equilibrium, which are apparent in epeirogenic warping, but from time to time the increase in strain becomes such that the margins of the lighter areas, continents, are crushed in, and an episode of orogenic movement ensues.

These effects of warping and mountain-building thus result from the mobility of the lithosphere, which is, however, conceived to be highly rigid in relation to sudden stresses or when taken in small masses.

A sufficient mass to account for all the superficial variations of form that are known is found within a depth of 70 to 100 miles below the surface, the geologic results being thus in accord with the determinations by Hayford of the depth of the zones of isostatic compensation. The inner mass of the earth, 3,900 miles radius, is conceived to be held in a state of relatively great stability by the high rigidity induced by pressure of the superincumbent load, and it is thought to graduate into the relatively unstable superficial layer through decrease of rigidity. The great nucleus itself is conceived to be the source of those energies which are expressed in variations of temperature and molecular condition through which the tangential strains gradually develop.

R. L. FARIS,
Secretary

THE AMERICAN CHEMICAL SOCIETY. NEW YORK SECTION

THE seventh regular meeting of the session of 1907–8 was held at the Chemists' Club, 108 West 55th Street, on April 10.

The Wm. H. Nichols medal, awarded annually for the best paper read before the section, was presented to Professor William H. Walker, of the Massachusetts Institute of Technology, for his paper on "The Corrosion of Iron and Steel."

The ceremony of awarding the medal was witnessed by many prominent members of the society. The chairman of the section, Professor Sherman, after reading a letter from the donor of the medal, Dr. Nichols, who expressed his regret at not being present, spoke of the great diversity in the subjects of research for which the authors had received the medal. These included agricultural chemistry, investigation of rare elements, organic synthesis, technical chemistry and finally a physico-chemical investigation of the properties of one of the most important metals.

The president of the society, Professor Bogert, in presenting the medal, gave a short history of the professional career of the recipient. He said, in part, that W. H. Walker graduated from Pennsylvania State College in 1890 and took his doctor's degree at Göttingen about a year later. Since then he has been professor of industrial chemistry at the Massachusetts Institute of Technology and lecturer on that subject at Harvard. Outside of his teaching he has been very successful in technical work, having made improvements in the manufacture of cut glass, in annealing silver, in brewing processes and in the preparation of wood products. He has also contributed much to the chemistry of cellulose.

Professor Walker, in acknowledging the medal, gave a résumé of the paper for which he received the award and outlined further investigations on the same subject which are now in progress.

The remainder of the evening was devoted to papers by Chas. Baskerville on "Courses in Chemistry for Colleges," and Jerome Alexander on "The Effect of Colloids on Crystallization: the Function of Gelatin in Ice Cream"—preliminary paper.

C. M. JOYCE,
Secretary

DISCUSSION AND CORRESPONDENCE

THE INTERPRETATION OF THE BEHAVIOR OF THE LOWER ORGANISMS

In my recent volume on the "Behavior of the Lower Organisms" are set forth certain views which have called forth discussion and criticism.[1] In response to many questions which indicate that there is a general interest in the subject, I wish here to reexamine some of the matters raised.

Objections have centered about my discussion of certain general theories of behavior, particularly of the tropism theory, and of certain applications of the theory of selection. Some of the criticisms are clearly just; others seem to me to rest upon misunderstanding, while still others show actual differences of opinion. To set in a clear light these different categories is my present wish.

The only question of importance is: How far is there a real difference of opinion, among workers familiar at first hand with the phenomena, in regard to (1) the actual, experimental facts of behavior, (2) the general and important laws or principles underlying these facts. Divergences due to different lines of interest, different fields of investigation and different understanding of terms merely obscure the essential point and need to be cleared away. To make clear the objects and meaning of different investigators sometimes reveals harmonious diversity in place of conflict; when this result is not reached, it at least shows precisely where opposition lies, and suggests experimentation that shall turn opposition into agreement.

It will greatly facilitate the attainment of these ends if I first set forth briefly certain purposes and principles that guided me in the preparation of my account.

1. My book was designed mainly as a topographic survey of the field of behavior in the lowest organisms. My primary purpose was to give the reader a clear idea of the observable facts, so that he might gain somewhat the same impression that he would from seeing the phenomena themselves. To aid in this, I characterized as typical such phenomena as were fairly representative of the behavior in general, while phenomena that were rare or unique, like the reaction to the electric current, I said were not typical and gave an inadequate idea of the nature of behavior. This, while strictly true, has led to misunderstanding; certain critics have assumed that I considered such phenomena as of no importance *from any point of view*. I believe that there is nothing in my treatment that gives ground for this assumption. Special cases of this form of criticism will come up in later paragraphs.

2. The most important characteristics of behavior have always seemed to me those shown in the biological interrelations of the physiological processes: in the relations of behavior to preserving the organism, to supplying the requirements for metabolism, and in general to keeping the other physiological processes in progress. These adaptive or regulatory characteristics of behavior furnished the problem to the solution of which attention was in my book mainly directed. Therefore I characterized as "important," "significant," and the like, mainly those features of behavior which seemed to lead toward an understanding of its regulatory character. Other investigators, not having this problem in the center of interest, have considered quite other matters as the important ones. Thus Torrey (*l. c.*) holds that we find the most important features of behavior in precisely those features that are not regulatory. Judgments of importance are of course relative; importance *for what?* is the question. I take it that the question in which Torrey is primarily interested is that regarding the nature of the immediate change which occurs in living matter when an external stimulus acts upon it.

3. We now come more directly to the content of the work. Years of investigation had convinced me that the complexity of the problems of behavior had been underestimated; that even in the lower organisms we are compelled to deal with an immense number of

[1] See especially Torrey, in SCIENCE, September 6, 1907; Loeb, *The Journal of Experimental Zoology*, Vol. 4, No. 1, 1907, and *Pflüger's Archiv*, Bd. 115, 1906, p. 580; Parker, SCIENCE, October 25, 1907.

complex and little-understood factors, rather than with a few simple ones.

In my book I therefore attempted, by citation of precise experimental data, to show the great number of factors which play a part in determining behavior; to show that changes in the internal system which makes up the organism are of equal importance for this, with changes in the external system which makes up the environment; and I set forth particularly those remarkable relations of dependence and support between the acts of behavior and the other physiological processes, that are commonly spoken of as adaptation.

4. My positive contention that behavior in the lower organisms is complex, varied and variable, so that it is not easily predictable, led me to a criticism of theories which represented such behavior as simple, uniform over wide fields, and unequivocally determined by single external factors. I found a theory of this character to be widely held; I met it in opposition at every turn as my papers began more and more to present behavior as complex; and I found this view presented regularly under the name of the "tropism theory."

This then was the reason for my attack on the tropism theory. I criticized it, not as a mere statement of one of the factors that make up the complex phenomena of behavior, but as a view of supposed extremely wide applicability, which maintained the simplicity and uniformity of the behavior of the lower organisms. I tried to show that there was no single schema into which most of the behavior of the lower organisms could be forced. How far it was just to identify the view criticized with the tropism theory we shall inquire in a moment; here it is most important, if we wish to get a clear understanding of the grounds of apparent conflict, to grasp the fact that the *simplicity, uniformity* and *general application of a single schema* were the points against which my criticism was directed.

Was this idea of the tropism theory sufficiently general to justify a criticism of it on that basis? The word tropism has been used in many senses and the theory has taken many forms, as we shall see later; but I believe that any one who has followed the literature of behavior must realize that there was such justification, even though he may himself hold to some other definition of the word tropism. The great movement toward extreme simplification in these matters has certainly been generally identified with the tropism theory; "reduction to simple tropisms" has been the ideal. Doubtless not all investigators have held the tropism theory to be so simple and of such wide applicability, but it is true that there has been a general belief that such was the case—a belief not confined to the uninformed, but shared by workers of high standing. Thus, Bohn, in his recent admirable review[1] of this entire question, after setting forth in detail examples of the tropism theory, says, "It is evident that nothing is simpler than this explanation," and again, "For more than ten years certain biologists have thus explained the actions of animals by tropisms. . . . This had become the *necessary* and *sufficient* explanation of all cases. Whenever it was observed that animals accumulated at a point, without even seeking to determine how they reached that point, a tropism was made to intervene." Bohn makes these statements merely as a presentation of well-known facts, and it would be easy to multiply quotations from biologists of the first rank showing that this idea of tropisms was a general one.

In view of certain passages in Professor Loeb's recent paper,[2] a note of historical character is here required. My criticisms have been directed not against any person or school, but against a prevalent view. I have never considered any single person as the sole author or only proper expositor of the tropism theory, but have taken the theory as I found it commonly presented in biological literature. I have not, therefore, considered it necessary to accompany a statement of my results with an exposition of Loeb's work and views; there are other authors whose work and friendship I value highly whose expressed views are more directly in opposition with what I have pointed out than are Loeb's. Certainly many authors besides Loeb have ventured on inde-

[1] *Journal of Experimental Zoology*, 4, 151–156.

[2] "Les Tropismes, les Réflexes et l'Intelligence," *L'Année Psychologique*, T. 12, 1906, pp. 137–156.

pendent contributions to the tropism theory and on expositions of it; if it was an error to take these into consideration, I am guilty of that error. Here we are seeking to discover whether there is divergence of view as to the facts themselves, and nothing would be more gratifying than to find in Professor Loeb an ally instead of an opponent, in this question of the complexity of behavior. We may, if we desire, call the theory which I criticized the popular tropism theory.

Now, how did this popular tropism theory simplify behavior? There is among investigators an extraordinary diversity of opinion as to what a tropism is. Some use the word as a mere name for certain observed facts. In reading, conversation and correspondence I have met the following definitions, each held by well-known investigators: (1) any reaction of a lower organism is a tropism; (2) any reaction to the chemical or physical agents of the environment is a tropism; (3) any movement toward or away from a source of stimulation is a tropism; (4) a tropism is any reaction in which the organism turns as directly as possible toward or away from the source of stimulation; (5) a tropism is any turning produced by stimulation; (6) a tropism is any reaction in which orientation to a steadily acting external force is the main characteristic. It seems clear that there is no tropism "theory" in any of these views; they merely apply a name to certain facts, leaving the nature of the reaction to be determined by experiment, and permitting different explanations in different cases. I myself at first used the term (or its equivalent "taxis") in some such collective sense, till a paper from the laboratory of one of the leading exponents of the tropism theory set forth with some warmth that the phenomena I described had "nothing to do with the tropisms."

Among those who use the word tropism in a precisely defined sense, implying a theory as to the nature of the reaction,[4] there is likewise diversity of view. The theories held by certain investigators give no ground for considering the tropism a simple, elementary phenomenon, nor one of wide application to lower organisms; they involve a highly developed sensory apparatus and a complex activity of the nervous system. Against such theories my criticism was not directed. On the other hand, there *is* a widely prevalent theory of tropisms which if correct really justifies the common view of the elementary simplicity of these phenomena. This is the "local action theory of tropisms," and it was against this that my criticism was directed.

I wish to emphasize this point, as it gives the key to the entire discussion. I found the fountain head of the commonly held belief in the simplicity and uniformity of the behavior of lower organisms in the "local action theory"—representing the stimulus as producing its reaction in that part of the body on which it directly falls, so that the organism reacts as a bundle of independent parts rather than as a unit. I therefore attacked this theory, and no other, in the chapter of my book which deals with this matter. I believe I made it perfectly clear that this was the theory under criticism; in the title of the chapter the "local action theory of tropisms" is specified; I defined precisely what I meant by it; all through the chapter I took pains to specify it, and in my summing up I expressly

my statement of the tropism theory on page 94 of my original paper is erroneous. The essential point in my characterization of the theory on that page seems to be the statement that "the theory of tropisms says that certain definite things happen in the change of position undergone by organisms under the influence of stimuli; that the organisms perform certain acts in certain ways." If this is the point which Loeb holds to be erroneous, my criticisms of course do not touch his views in the least. Many authors present the tropism theory as a theory of how reactions occur, and it was as such that I criticized it. If I have anywhere wrongly classified Professor Loeb with these, I regret it, and am delighted to discover my mistake. Any one who holds a theory (or would be a *theory?*) of tropisms that says nothing as to how the reaction occurs will hardly find anything in my discussion to oppose his views.

[4] I am uncertain whether Professor Loeb, in his recent paper (*Journ. Exp. Zool.*, **4**), wishes to range himself with those for whom the word tropism implies nothing as to the nature of the reaction. In his note on page 156, he says that

named the local action theory. I believe there is no ground for misunderstanding the theory that I was criticizing, though this seems to have occurred in certain cases.

My discussion has been attacked from two sides. In one recent number of SCIENCE Parker[5] takes the ground that the local action theory is not held, so that it was not worth while to demolish it; while in another recent number, Torrey[6] expressly defends the local action theory. These mutually destructive criticisms naturally relieve me of some embarrassment in replying to both. Torrey's elaborate defense shows that the theory is still very much alive and I can, therefore, only greet with pleasure Parker's ready support of my main contention, even though this takes the form of Sairey Gamp's crushing retort, "Who deniges of it, Betsey? Who deniges of it?" Parker's work has been mainly with more complex animals than those dealt with in my book, and his interests have lain rather in the field of sense physiology than in the development of activity. I can, therefore, readily understand that he should find it inconceivable that such a view should be held; he has doubtless not met it in opposition at every turn, as have those working with the lowest organisms, and as I now meet it in Torrey's paper. In my book I have given precise statements of the theory in the form of quotations from authors of highest standing. Bohn[7] in his recent exposition adds others. None of the authors quoted has, so far as I am aware, repudiated the local action theory. It would appear, therefore, that a statement of the relation of the observed facts to this theory was much needed.

Before turning to the arguments urged in support of the local action theory, another criticism of my discussion, made or implied by most of my critics, must be dealt with. This may be put as follows. Suppose that the simple, local action theory of tropisms is not satisfactory. Nevertheless, there is another, less precise, less simple, theory of tropisms which is of itself important; a theory in support of which Parker and Torrey cite the circus movements of animals when the sense organs of one side have been obstructed. A theory of such importance, it is contended, should have been dealt with in a general work on the behavior of the lower organisms. Further, the chapter criticizing a theory under the name of tropism gives the impression that this other theory is also condemned, though arguments against it are not advanced.

To this criticism my book is justly open. I should have given an exposition of the theory in question, with an attempt to estimate the part it plays in the behavior of the lower organisms. This unpurposed omission was partly due to the fact that the two groups of whose behavior I gave a detailed exposition—the Protozoa and Cœlenterata—have furnished practically none of the evidence cited by my critics; partly to my attempt to focus attention upon the local action theory as of infinitely greater importance than the other form of the theory. But even though I held that action in accordance with the complex form of the theory plays little part in the behavior of the lower animals, the phenomena and theory should have been set forth, and I regret that this was not done.

We may now return to the criticisms and defense of the local action theory. Regarding the nature of my criticisms, one point must be emphasized—a point that has been much misunderstood, though I believe I expressed myself explicitly on the matter. I made no attempt, and had no desire, to deny the existence of the factors on which the local action theory, or any other existing theory of tropisms, was based. So far as local action is concerned, I emphasized in my book such cases as were established, and gave a list of them on page 306. The question which I tried to answer in my discussion of tropisms could be put thus: After some years of study of the behavior of the lower organisms, what is your impression regarding the extent and importance of the part played by tropisms? A well-known investigator, after one of the most thorough and detailed studies of the behavior of a certain group of invertebrates that have ever been made, in which he watched and ex-

[5] SCIENCE, October 25, 1907.

[6] SCIENCE, September 6, 1907 ("the response to stimulation is local," p. 319, etc.).

[7] *Loc. cit.*

perimented with the animals literally day and night for long periods, remarked to me, in discussing this matter, that he never saw any tropisms. Without going so far as this, my answer to the question asked above was that "the theory of tropisms does not go far in helping us to understand the behavior of the lower organisms." I did not deny the existence of the phenomena which the theory takes into consideration, but it seemed to me that there are so many other factors, playing such important parts, that the tropism factor is of relatively small importance in a general consideration of behavior. In my first paper on this subject[1] I included the more complex forms of the tropisms in this judgment. The remark of the investigator above mentioned illustrates the fact that there are certainly other aspects of behavior so striking and important as to quite mask the existence of tropisms.

Let us attempt a brief characterization of tropisms, their history, and the part they play in behavior.

1. The essential point in the tropism, as originally applied by Loeb to reactions to light was, in a word, the idea that the organism in going toward or away from the light is not trying to go somewhere or to reach something, but is merely taking a certain position or *orientation* in the light. This recognition that the *position* is the essential point was a great step in advance, and its application by Loeb to certain features of the behavior of animals was an achievement of the highest importance.

2. This idea of orientation having proved so helpful in the study of reactions to light, the next step was, very properly, to apply it to other features of behavior, to see if it would not prove equally useful elsewhere. The reactions to chemicals, heat and cold, contact with solids, electricity, light, gravity, etc., were all brought under this point of view; attempts to show that the position is the essential point in each of these have for a long time been made with energy and persistence. Our knowledge has grown till we are in a position to estimate the results. In the main it appears that to most of the behavior the orientation idea has little applicability. To that immense province of behavior comprised in the reactions to chemicals of all sorts (including food reactions, respiratory reactions, etc.) it has shown itself quite inapplicable. The case is the same with the reactions to heat and cold. With regard to the reactions to solid bodies nearly the same may be said, though there are some special cases in which the idea of orientation is applicable. The reaction to the electric current furnishes a typical orientation. In some of the reactions to gravity and to water currents the orientation idea is helpful. Yet the recent work of Lyon and others shows that even in these the movement in a certain direction is an essential part of the reactions; they are essentially compensatory movements, and the taking of a certain orientation is by no means the only important point. In certain reactions to light the orientation idea has been most helpful, yet in an immense proportion of the reactions of organisms to this agent it does not show itself the essential point. The orientation theory is of greatest service in such cases as the going of insects toward a bright light, yet even here such work as that of Holmes on Ranatra shows that the orientation is not the only point; the approach to the light seems after all essential, since if a certain orientation does not bring the animal to the light, it learns by experience to take a different orientation which does have this effect. The work of Cole, of Radl and others, shows that in the lower organisms we have the beginnings of reactions to *objects* perceived visually; the animal is not merely oriented by the strongest light, but goes toward such objects, whether bright or dark, as might be said to be "of interest"[2] to it at the given

[1] "Contributions to the Study of the Behavior of the Lower Organisms," Carnegie Institution, Publication 16, pp. 89–107.

[2] The expression "of interest" of course has some objective equivalent, but to try to use it would be to substitute an unintelligible conjecture for an expression which at least conveys an idea

moment. The reactions to light are bound up with almost every possible aspect of behavior, and the orientation principle plays in them but a relatively small part.

3. In Loeb's original theory nothing was said as to the way in which the position of orientation is reached, and I take it that he does not now consider this matter as belonging to the theory proper. But on this matter a tempting idea presented itself, to the effect that the position of orientation was reached in the simplest possible way—by a local reaction of the part on which the stimulating agent impinges. This gave the "local action theory" which made the tropism a thing of such extreme simplicity; it has been applied, in detail or in general, to all sorts of reactions, by many authors. While it holds in a measure for the effect of the continuous electric current, I believe that it has been demonstrated that in the main this idea was not correct; that the element it deals with plays little part in behavior, aside from the effects of electricity. With this we shall deal in later paragraphs.

Attempts have been made to controvert my position on the tropism theory by the performance of crucial experiments or by the citation of specific observations. These are clearly based on misunderstanding. It is obvious that the relative importance of an admitted single factor in producing a set of complex phenomena can not be settled in this manner. Valuable judgment on such a question can be based only on an extended study of the phenomena. My own opinion derives any worth it may have solely from the fact that I had worked for nine years on the behavior of a large number of organisms, attempting to make a careful analysis, with detailed studies of the different factors involved and the part played by each. My conclusions are of the same character as are drawn from a large mass of statistical data. They can be adequately controverted only by showing that the analysis of this mass of data, or of another equally large or larger, will not yield these conclusions. Single observations are of course important, since they are the material from which the large mass is made, but single observations taken by themselves do not help much in taking off the *facies* of a long series of investigations, which is what I tried to do. My conclusion, like all statistical conclusions, is nothing that will enable one to predict for a given individual case; if it were, it would of course be of much greater value than it is. No single observation whatever is inconsistent with my general conclusion.

Thus, writers who have flown to the defense of the *existence* of tropisms will find themselves in no conflict with my stand on the matter. It was only the prevalent opinion of the wide generality and importance of the phenomena that I called in question. To hear that the actual existence of the tropism was held to require defense came as a real surprise to me. If put forward as merely one factor out of many, with its relative importance subject to discussion, I shall agree most cordially.

In certain quarters there seems to be an impression that observation of the direct turning of an organism toward a source of stimulation is in some way opposed to my views, and that citation of specific cases of this will come to me as a painful surprise. Yet, of course, this is one of the commonest and most evident facts of behavior, and is discussed in detail in my book (see, for example, pp. 306–308). Its existence is required if the theory I suggested is correct. I pointed out that in consequence of the three factors in behavior whose importance I emphasized, this direct turning toward a localized stimulus would occur; if it did not occur, that would tend to disprove the theory. "Innumerable instances of this class of reactions could be given; they include perhaps the greater number of the directed movements of organisms" (p. 307).

Thus the direct turning of animals is not in dispute. A matter that *is* of interest lies in the answer to the question whether the turning is due to the simple local reaction of the region on which the stimulating agent impinges. My own contention was that this is rarely the case. If authors will state clearly whether

of the outward facts. The animals go toward visible things that serve for food, protection, etc.

they conceive the turning to be due to such simple local reaction, and give the evidence on which this opinion is based, that will be a real contribution on a disputed point.

The reaction to the electric current, in which the effect is local and the behavior is uncoordinated and unadaptive, is the type and pattern of the local action style of behavior. Its importance is thus naturally emphasized by Torrey, in his defense of that theory, as against my own contention that this reaction is not typical of the behavior of lower organisms. The question will be cleared most readily by noticing the different objects which guided us in taking our stands. My own purpose, in my topographical survey of behavior, was to give the reader a correct idea of the facts—of what he would see if he examined the phenomena himself. In doing this, one must inevitably come, I believe, to my conclusion that the "action . . . under the electric current is not typical of the behavior under other stimuli." If the reader examined accurately the reaction to the electric current he would see certain phenomena—local action, lack of unity and coordination, different parts of the body opposing each other, etc. The question is—Is this typical of the behavior? Is this what would be seen if the reactions to heat, light, gravity, chemicals, etc., were examined in the same way? Certainly it is not. If the reader should get the impression that the extraordinary series of phenomena seen when an electric current is passed through a collection of infusoria is likewise what is seen when they are subjected to other stimuli, his idea of behavior in the lower organisms would be a ridiculous caricature of the reality. There appears to be no reason for concealing this fact, and I set it forth as clearly as I could.

On the other hand, Torrey holds that in the reaction to the electric current we may have exhibited in a very direct way some of the fundamental changes that occur in living matter when subjected to the action of a stimulus; hence its great importance. Nothing that I have said militates against this opinion. The statements and implications that I hold that "the uniqueness of the electric stimulus . . . vitiates its claim to consideration"; that "the interesting phenomena of galvanic stimulation are to be so lightly put aside," etc., emanate from Torrey, not from myself. So far have I been from "neglecting" it, that I devoted in my book more space to this reaction than to any other. But the importance of the reaction to electricity is of the same sort in the behavior of lower as in that of higher animals; though of the utmost importance, no one would consider the reaction of a muscle to electricity "an adequate type of the behavior of mankind." I believe that it was made plain in my book that this was the point which I was setting forth.

Torrey takes up my account of the reactions of *Euglena* to light, and attempts to show that it agrees with what would be expected from the local action theory of tropisms. It is not possible to take up the details of this matter here. But I may point out the following: In accordance with my general practise, my account in this case was based, not on an attempt to explain an isolated reaction by a preferred theory, but on an extensive analytical investigation of the reactions of the organism, attempting to isolate experimentally the various elements of which the behavior is made up. In this investigation I was not able to find experimentally that element which the tropism theory calls for, while those I did find accounted for the entire behavior. I, therefore, had no ground for asserting the existence of the tropism element. I do not see that Torrey has adduced any additional ground for such assertion; at best he has merely tried to show that interpretation along the line he prefers is not inconsistent with the facts.

One of my figures of the reaction (Fig. 93 in my book) Torrey thinks "perfectly in harmony with the tropic schema," he says: "it is hard for me to conceive how an organism swimming of necessity in a spiral course could react more definitely to a moderate directive stimulus than *Euglena* does here"; and he "can only wonder at my running so boldly and far into the enemy's camp," Surely this last remark does not mean that Dr. Torrey considers it a reputable

scientific procedure to manufacture or alter a figure claiming to represent the facts, in order to make it agree with a theory. My figures were made from as precise a study of the facts as I could make, before I had attempted by analysis of all the facts to see what they mean, so that the figures form part of the data for my later conclusions. In my preface I said that my ideal was "to present an account that would include the facts required for a refutation of my own general views, if such refutation is possible," and I should not be without the gratification of having fulfilled that ideal if Torrey should be adjudged to have made out his case. But the reason why I held that the organism does not react as directly as possible is as follows: The oriented organism is swimming toward the light in a spiral course, thus swerving first to the right, then to the left (omitting from consideration the movement in other planes). Now the light is changed, so as to come, say, from the right, as in my Fig. 95. The most direct way in which the organism, swimming in a spiral, could become oriented to the light would be by an increase in the swerving to the right and a decrease in the swerving to the left, and this is what the tropism theory would lead us to expect. But the fact is that there is an *increase* in the swerving both to the left and to the right, the spiral becoming a wider one; the increase to the right being, however, greater than that to the left, the organism becomes gradually pointed to the right. The increased swerving to the left is not accounted for by the tropism theory, and is indeed squarely opposed to it, while it is to be expected if the analysis I gave is correct.

The point becomes quite clear when we compare this reaction with that to the electric current, which with its undoubted local action Torrey considers a typical tropism. Since *Euglena* itself has not been shown to react to electricity, we can not make the comparison here, but Torrey does not maintain his views for *Euglena* alone, and the facts in the reactions of ciliates to light, gravity and water currents are parallel to those in the reactions of *Euglena* to light. In all these reactions the organism swerves, in becoming oriented, only toward a certain side x, never toward the opposite side y, just as in the reaction of *Euglena* to light. But in the reaction to the electric current the organism may be caused by the local action to swerve directly toward the side y, and to become oriented in that way. Local action would cause swerving toward the side y in the reactions to light, gravity, etc., exactly as in the electric current, and the fact that this does not occur seems to be a demonstration that local action is not the explanation in these cases.

It will then be clear, I hope, that my analysis was based on a thorough consideration of the available experimental data, and not on prejudice for or against any given theory. Torrey indeed admits, if I understand him, the existence of all the factors which I set forth, and the correctness of my analysis so far as it deals with positive factors, but believes that there is an additional factor, in virtue of which *Euglena* may turn *directly* toward a light. Thus the behavior of *Euglena* is more complex, according to Torrey's view, than I represented it. There is no doubt but that increase of knowledge tends to reveal increased complexity in the behavior of the lower organisms; of this many recent examples could be given. My own work has had decidedly this tendency, but, as in the present case, I tried to keep the theory as simple as the facts would permit. But my experiments on *Euglena*, while not revealing the power of direct turning, do not disprove its existence. It has always seemed as extraordinary to me as to any one else that the direct turning should not occur. There is little profit in discussing matters which only experiment can settle. At the time my work was done, I had no opportunity to study the reactions of *Euglena* in the stage when it has no flagellum and moves by contractions. Such a study is much needed, and it may reveal the additional factor which Torrey looks for. Many higher organisms show a power of direct localization, in connection with complex activities of other sorts; there

is no antecedent improbability of this in *Euglena*.

Related to this matter is Torrey's discussion of the question whether the organism is or is not stimulated after it is oriented, which leads him finally to the extraordinary conclusion that I "insist on an interpretation of organic behavior by means of general changes in internal states that are psychical rather than physical," and to a general condemnation of my analysis on this account. Most of the points made in Dr. Torrey's interesting paper I can appreciate, but at the discussion which leads to this conclusion regarding psychic factors I must confess my astonishment. The conclusion is reached only by the aid of the somewhat desperate assertion that to say that an oriented organism is subjected to no general stimulation "is no more than saying it then possesses no feeling of discomfort." Had I made such a statement, I should have expected much just and severe criticism for "psychologizing"; for "crude anthropomorphism."

The root of the difficulty lies in a misunderstanding of certain of my attempts to avoid the use of indefinite terms not having a precise experimental meaning; it comes finally to a simple matter of definition. Experimentally, it has seemed to me that the study of behavior reduces mainly to a study of two things: (1) the causes of changes in behavior; (2) the nature of the changes themselves. Now these two things correspond nearly to what are commonly called *stimuli* and *reactions*, though the common usage is a little less precise, not always representing experimental concepts. I, therefore, adopted for experimental discussions the word reaction as signifying a *change in behavior;* the word stimulus as meaning the *cause* of a change in behavior, though so far as I could I used the plain phrases in place of the two terms. Unless some such definitions are used there is no experimental method of telling whether an organism is reacting or not; whether it is stimulated or not. On page 283 of my book I took the greatest pains to emphasize the fact that my discussion would not be intelligible unless this meaning of the word reaction were kept in mind. With this, a stimulus, as the cause of a reaction, is likewise clearly defined; this definition I had already given on page 6.

Stimulation and reaction are evidently, as thus used, correlative terms; if there is no reaction, there is no stimulation. I have no desire to insist that these are the only possible definitions; I merely wish to point out that this was my explicitly declared usage. Now, if we apply these definitions, the whole structure of difficulties raised by Torrey falls to the ground. From the definitions it follows that when the movements of an organism are uniform, it is not stimulated. After the infusorian has become oriented to light, it does not change its movements, but swims in the same way it did before; there is then no external evidence that it is stimulated, and if my purely empirical definition is accepted, it is not stimulated.

If it be maintained, as Torrey does, that the organism is nevertheless stimulated at such a time, then evidently some internal condition is taken as a criterion of stimulation. This is precisely the criterion which Torrey incorrectly attributed to me, and on the ground of which he charged me with making "feelings of discomfort" and other psychical phenomena the basis of my analysis. If there were any sound foundation for his argument, I could retort that it is his view that calls for the psychical factors. But, of course, there is no reason for dragging in psychic factors at all; it is perfectly easy to suppose that the organism when oriented is in a differing physico-chemical state, and this assumed state might be considered stimulation, unless the empirical definition of stimulation as correlative with reaction is preferred. To be unable to conceive a change in physiological state otherwise than as psychical would seem to unfit one completely for the objective analysis of behavior; such changes demonstrably occur even in unicellular organisms.

It is evident that the highly objectionable propositions which Torrey deduces from my discussion, to the effect that there can not be "a constant stimulus that does not induce

a differential movement," or that I deny the "possibility of symmetrical stimulation for an oriented organism," depend simply on the definition of stimulus and reaction as correlative terms of purely experimental meaning. I certainly believe that many animals, after they have "fixed" a source of light and are swimming toward it, are in a different physiological state from before. If we define stimulation somewhat indefinitely as meaning any such changed physiological state, then we may of course hold that they are then stimulated. There seems to be no real difference of opinion on this matter; but the method of formulation of course depends on the definition of the terms employed.

A further point discussed by Torrey has to do with the relation between selection and adaptation. As an aid to understanding the existence of adaptations in behavior, I accepted certain forms of the selection theory. Torrey emphasizes the existence of unadaptive reactions, like that to the electric current; he points out that there is no ground for supposing that selection has played a part in their production. To this I agree fully. But since Torrey draws therefrom the conclusion that "the hypothesis advanced by Jennings is not sufficiently broad to encompass all the phenomena it is devised to explain," it needs to be pointed out that my view was not "devised to explain" such phenomena. A theory of selection, while directed primarily to the explanation of adaptiveness, requires the existence of raw material from which selection may occur, and this raw material must of course be largely unadaptive, or there would be no ground for selection. Selection can never account for the existence of that from which selection is made. This, I believe, was made plain in my book. "It is clear that natural selection can not account for the origin of anything; only that can be selected which already exists" (p. 326). I stated explicitly that the hypothesis set forth was a theory *of regulation;* my exposition of the matter opens on page 315 as follows: "The question in which we are interested is then the following: How can behavior develop? That is, how can it change so as to become more effective—more regulatory?" The existence of unadaptive reactions not coming under this theory was recognized. "The organism is composed of matter that is subject to the usual laws of physics and chemistry. External agents may of course act on this matter directly, causing changes in movement that are not regulatory" (page 345). The origin of these unadaptive reactions I did not discuss, because I had no light to throw on the matter. But I emphasized my conviction that the study of the laws of matter and energy furnish the main field for investigation, as compared with questions of selection. "Whatever the part assigned to natural selection, the superlative importance of these laws remains; they must continue the chief field for scientific investigation" (p. 326). I might have said "the only field," since of course the study of selection is merely the study of how these laws work under certain complicated conditions.

Torrey evidently overlooked my explicit statements of the object and limitations of the theory in question.

This discussion of theories of development tends to give the impression that these form the important part of my treatment of behavior. It is, therefore, only just to point out that this matter was a side issue from the main purpose of my work, and was explicitly put forward merely as a suggestion as to what may have occurred. The short chapter on this subject begins as follows: "It is not the primary purpose of the present work to treat the problems of development, but rather to give an analysis of behavior as we now find it. But the results of this analysis furnish a certain amount of evidence as to how development may have occurred; this it will be well to set forth briefly." The book is primarily a treatment of behavior as a branch of experimental physiology.

But I believe it to be short-sighted and unfortunate for a physiologist to attempt to set in opposition physiological interpretation, on the one hand, with so-called "historical" interpretation, dealing with selection and evolution, on the other. Selection is not something outside of physiological or physico-chemical

action; it is merely a characterization of certain ways in which such action occurs. Some of the relations brought about in such action are lasting, while others are fleeting; those which last are said to be "selected." The study of selection is an examination of the relative permanency of different physico-chemical and physiological relations, and it is eminently a matter for experimentation.

Again, in addition to the rapid processes occurring mainly in the lifetime of individuals, there are slow processes requiring more than a generation to produce evident effects; various aspects of these we call "heredity," "evolution," "genetics," etc. These slow processes belong as much to physiology as do the rapid ones. The existing condition of living things is known to be largely a product of these processes, so that to attempt to exclude them from consideration and to act as if their effects did not exist, when we are trying to understand living things, is a most futile proceeding. These matters are coming rapidly under experimental study, so that attempts to exclude them from consideration in physiology, as "historical," can not endure much longer.

We now come to the matter which seems to underlie most of the criticisms of my discussion. Certain authors seem to identify the "tropism theory" with the view that the behavior of organisms is to be explained by objective, experimentally determinable factors. They feel that an attack on the "tropism theory" is an attack on this view; this comes forth notably in the criticisms made by Loeb and Torrey, and it is evident in the attitude of some other writers.

There is, so far as I can see, nothing in the facts and relations which I have brought out that in any way opposes the principle that behavior is to be explained by objective, experimentally determinable factors—or indeed that bears in any way on the question. I have simply assumed throughout that it *is* to be explained in that way, and I do not see how experimental investigations can proceed on any other basis. Beginning my work in 1896, when the movement led by Loeb against the use of psychic concepts in explaining objective phenomena was in full swing, I considered that battle as fought and won; I have, therefore, ever since proceeded, without discussion or ado of any sort, on that basis. Every one must recognize the tremendous service done by Loeb in championing through thick and thin the necessity for the use of objective, experimental factors in the analysis of behavior. No convinced experimentalist, knowing the previous history of the subject, can reread, as I have just done, Loeb's early work on behavior without being filled with admiration for the clear-cut enunciation, defense and application of the principles on which valuable experimental work has rested since that time, and on which it must continue to rest.

Any differences of opinion between Loeb and myself are then matters of detail; they concern merely the results of the application of the agreed principles of investigation. It has seemed to me that some of the experimentalists have rested content with superficial explanations; that they did not realize the complexity of the problems with which they were dealing. This has been the history of most applications of experiment to biology; the more thorough the work, the deeper are the problems seen to be.

Thus I have not hesitated to bring forth facts tending to show the inadequacy of the physico-chemical factors thus far set forth, and doubtless some have suspected that this was done with the concealed purpose of discrediting the general adequacy of such factors. This is a complete mistake; I did not till lately realize even the existence of such a suspicion. Complete confidence in the experimental method removes anxiety as to the effect of criticizing the details of its application. My objections are only to the adequacy of particular factors; they are based on experimental grounds, and the difficulties they raise are to be resolved only by experimental study. There is a vast difference between holding that behavior is fundamentally explicable on experimental grounds, and holding that we have already so explained it.

In a recent paper Loeb[1] has intimated that even if the behavior of the organisms under consideration were as complex as that of man, the same objective and experimental methods must be used in analyzing it. To this I fully agree, and the behavior of man is of course no more to be excepted from this treatment than is that of any other organism. In some recent writings one finds indications of a curious dualism, as if the behavior of lower organisms were to be analyzed in the objective, experimental way, but the behavior of higher animals and man were not. This takes most often the form of objection to any comparison between the objective features of the behavior of higher and lower animals, or to the use of the same terms in speaking of them, with a tendency toward accusations of vitalism or "psychologizing," against those making such comparisons. Such accusations evidently depend on the premise that the behavior of higher animals is to be explained only by vitalism or by "psychologizing." When one is tempted to accuse an opponent in such ways, it is worth while to first examine whether the tendency to read psychic or vital factors into the phenomena does not lie in the mind of the accuser, rather than in that of the accused. When one has consciously and consistently taken the ground that the behavior of all organisms, including man, is to be analyzed in the objective, experimental way, and that there is no ground for expecting a failure of this method at any point, there is less occasion for anxiety at the use of similar terms for similar objective phenomena throughout the series.

For example, the "method of trial and error" is as much an objective phenomenon, to be explained by experimentally determinable factors, in the dog or man, as in the infusorian. The undoubted great differences between the exemplifications of the "method" at the two extremes are matters for experimental analysis and demonstration, if the experimental method is not to fail. They do not necessarily show that the fundamental principle involved is different, and it is this common fundamental principle to which the common name calls attention. How far we should avoid words that have ever had any psychic connotation whatever is a matter on which there may be divergence of opinion; but it is most important to realize that this is totally distinct from the question whether the psychic connotation is of any use in objective experimental analysis. If this distinction is lost sight of, a divergence in practical details is taken for a conflict in fundamental principles, to the detriment of experimental science. That it is impossible to avoid such words completely is seen when we find in the writings of such men as Loeb the frequent use of such terms as "associative memory." Of course it is to only the objective phenomena that Loeb refers; but this is precisely the case also with other experimentalists accused of similar practises!

To sum up the discussion with the defenders of the tropism theory: We all stand on the same foundation, and the differences of opinion are in matters of detail. In attempting to demonstrate the complexity of the problems of behavior, I have focused attention on a certain precise and narrow form of the tropism theory which seemed to me to have gained undue prominence—in order to show that such narrow schemata are inadequate. In so doing other forms of the theory, more flexible in character, and setting forth the tropism as but one factor out of many, have been thrown into the background; of this the supporters of the theory have justly complained. With my main contention that behavior in the lower organisms is complex, involving many factors, so that no one schema gives an adequate account of it, there seems to be little disagreement. As to the value of the "local action" theory there is still divergence of opinion.

And now a word as to my own positive contributions to the analysis of the matter. It is obvious that conclusions of the "statistical" character that I have attributed to my own, are, from their relative inapplicability to specific cases, of much less value than precise

[1] *Pflüger's Archiv*, 1906, **115**, p. 581.

chemico-physical or physiological ones. It is also obvious that to demonstrate the complexity and difficulty of a field of work is not an achievement to be compared in value with the demonstration that this field is simple and easily explicable on a few known principles. I am under no illusion in regard to this. The clear-cut, narrow tropism theory would be of infinitely greater value for predicting and controlling the behavior of animals than anything I have offered, if only it were true. I am sure I regret that I can make no attempt to put an equally simple schema in place of the one I criticized; if the phenomena of behavior were of elemental simplicity, that would certainly be much more convenient, though perhaps they would then be less interesting. Many of the concepts used in my analysis—"physiological states," "selection," "trial and error," and the like—are collective ones, characterizing varied phenomena of a high degree of complexity. They all require much further analysis; they are programs for future work, not final solutions of the problems. My analysis was mainly an attempt to lay out the field, to point out the principal phenomena with which we have to deal, and to define some of the main problems. If any one attempts to explain all behavior on any one basis, to unlock all its secrets by any catchword whatever, be it "trial and error," "selection," "tropisms" or what not, he lacks a realization of the complexity of his field of investigation. Like other complex fields, that of behavior, even in lower organisms, must be divided up; the various factors must be subjected to long and intense special investigation, with a realization that we have here material for the work of many generations of investigators.

H. S. Jennings
Johns Hopkins University,
Baltimore, Md.,
November 26, 1907

SPECIAL ARTICLES

INTERPRETATION OF THE CHEMICAL COMPOSITION OF THE MINERAL BENITOITE

In July, 1907, Professor G. D. Louderback published an interesting paper on the new mineral benitoite. For a description of the physical properties of the mineral the reader is referred to the original article.[1] The analysis of the mineral, made by W. C. Blasdale, showed the following:

	A	B	Average	Mol. Ratios
SiO_2	43.56	43.79	43.68	0.723
TiO_2	20.18	20.00	20.09	0.250
BaO	36.34	36.31	36.33	0.237
	100.08	100.10		

This yields the empirical formula $BaTiSi_3O_9$. From this Louderback concludes: "Benitoite is then a very acid titano-silicate of barium and stands in a class by itself, both as regards acid silicates and titano-silicates."

Upon reading the paper, immediately after its publication last year, I noticed that there was a very striking similarity to be observed between the composition of benitoite and beryl, for, if benitoite be interpreted as a metasilicate, we have:

Beryl ..... $\overset{II}{Be_3}\overset{III}{Al_2}(SiO_3)_6$

Benitoite ..... $\overset{II}{Ba_2}\overset{IV}{Ti_2}(SiO_3)_6$.

This similarity in the chemical composition is sufficient to consider the two compounds as isomorphous, for, although titanium with a valency of four replaces aluminium with one of three, the total valences in both compounds are the same. There is, however, a difference of one with respect to the number of atoms. A few examples of well-known isomorphous series will show that the above is not unusual.

In the marcasite group we have:

Marcasite ..... $\overset{II}{Fe}\overset{II}{S_2}$

Arsenopyrite ..... $\overset{III}{Fe}\overset{II}{AsS}$

Löllingite ..... $\overset{III}{Fe}\overset{III}{As_2}$.

Here the number of atoms is constant in all three compounds, but the valences vary. The albite-anorthite group furnishes another illustration.

Albite ..... $\overset{I}{Na}\overset{III}{Al}\overset{IV}{Si_2}O_8$

[1] "Benitoite, a New California Gem Mineral," University of California Publications, Bulletin of the Department of Geology, Vol. 5, 149–153, 1907.

Anorthite ..... $\overset{II}{Ca}\overset{III}{Al}\overset{III}{Al}Si_2O_8$.

In this series the valences of some of the individual elements vary, but their total and also the number of atoms in each compound are the same.

The galena group illustrates an isomorphous series in which the individual valences and the total number of atoms may both vary.

Galena ..... $\overset{II}{Pb}S$

Argentite ..... $\overset{I}{Ag_2}S$.

(For other examples of isomorphous series see Brauns's "Chemische Mineralogie," Arzruni's "Physikalische Chemie der Krystalle," and Groth's "Tabellarische Uebersicht der Mineralien," preferably the French edition of 1904.)

The similarity of beryl and benitoite is also to be noted by comparing their axial ratios. Both minerals crystallize in the hexagonal system.

	a	*c*
Beryl	1	0.4989
Benitoite	1	0.4230.

The value for benitoite is one half of one of those suggested by Louderback and is, no doubt, to be considered only as approximate, since no data concerning the reliability of the readings are given.

It is well known that TiO_2 has either acidic or basic properties. In the case under consideration, the quantitative analysis simply reveals the presence of TiO_2 as such. As to whether it is acid or basic, depends upon which of these assumptions will allow of the simplest explanation and yet be in strict accord with the observed facts.

The similarity of the composition of beryl and benitoite indicates a metasilicate and that TiO_2 is to be considered basic in this instance. The metasilicates are very common in nature, which is not the case with the salts of $H_2Si_4O_9$, a derivative of the tetrasilicic acid $H_4Si_4O_{10}$, to which Louderback would refer benitoite. Up to the present time only one mineral, lorenzenite with the composition $Na_2(Ti, Zr)_2Si_2O_6$, has been observed which can be referred to $H_2Si_4O_9$. In lorenzenite, Ti and Zr replace two atoms of silicon, but in benitoite, according to Louderback's interpretation, one atom of silicon would be replaced by titanium. Thus, in both cases, $H_2Si_4O_9$ is to be considered the basis. Hence, even though Louderback's interpretation be correct his statement, page 152, "Benitoite . . . stands in a class by itself, both as regards acid silicates and titanosilicates," needs to be modified.

In a very recent paper by Ralph Arnold,[2] it is pointed out that benitoite occurs in an area of basic rocks, such as serpentine and glaucophane schists. In fact, benitoite is found in cracks and cavities in the latter and is always associated with the hydrated basic metasilicate natrolite, $Na_2Al(AlO)(SiO_3)_3.2H_2O$. According to Arnold, benitoite may have crystallized before the natrolite, but some occurrences seem to indicate that probably the crystallization of these minerals was practically contemporaneous.

Therefore, the interpretation suggested above, which is based (1) upon the similarity of the chemical composition and axial ratios of beryl and benitoite; (2) upon the fact that TiO_2 may act as a base; (3) that benitoite occurs in an area of basic rocks, and (4) is always associated with the hydrated basic metasilicate, natrolite, seems amply justified. Accordingly, from what has been published, benitoite is not to be considered "a very acid titanosilicate of barium," but rather a metasilicate of barium and titanium.

EDWARD H. KRAUS
MINERALOGICAL LABORATORY,
UNIVERSITY OF MICHIGAN

SOME PHYSICAL CHARACTERISTICS OF COLLEGE STUDENTS[1]

A PHYSICAL examination is required of all freshmen in Columbia College and the Schools of Applied Science during the first month of the academic year. The examination includes

[2] "Notes on the Occurrence of the Recently Described Gem Mineral, Benitoite," by Ralph Arnold, SCIENCE, N. S., Vol. XXVII., pp. 312–314, 1908.

[1] Read before the New York Academy of Sciences, Section of Anthropology and Psychology, on October 28, 1907.

a record of personal and family history, 31 measurements, 7 strength tests, vision and hearing tests, condition of skin, heart, lungs, nose, throat, teeth and of any abnormalities of spine, shoulders, chest, feet, etc.

The main purpose of these examinations is to help the medical director in guiding and advising each student in matters of physical exercise and hygienic living. The data collected are of interest to the physician and the anthropologist by showing the occurrence of various abnormalities and physical characteristics in healthy young men.

AVERAGE MEASUREMENTS OF 790 COLUMBIA STUDENTS 17, 18 AND 19 YEARS OLD

	First Exam.	Averages			Second Exam.
		206 Students	317 Students	267 Students	
Date					
Age		17.5	18.1	19	
Weight		58.29	58.36	60.32	
Stretch of Arms		177.86	178.13	178.36	
Height Standing		171	171.31	171.61	
Height Sitting		88.84	89.24	90.1	
Length of Trunk		63.67	63.9	64.63	
Girth of Neck		33.5	33.79	34.27	
Girth of Upper Chest—Repose		84.44	85.08	86.17	
Girth of Upper Chest—After Expiration		81	81.07	82.23	
Girth of Upper Chest—After Inspiration		89.53	90.06	91.2	
Girth of Lower Chest—Repose		78.2	79	79.7	
Girth of Lower Chest—After Expiration		76.17	76.94	77.52	
Girth of Lower Chest—After Inspiration		84.52	84.96	85.94	
Girth of Waist		69.5	69.86	71.00	
Girth of Right Upper Arm		24.54	25.14	25.4	
Girth of Right Upper Arm—Flexed		27.75	28.24	28.8	
Girth of Right Forearm—Contracted		25.7	26	26.5	
Girth of Left Upper Arm		24.14	24.5	25.1	
Girth of Left Upper Arm—Flexed		27.15	27.65	28.2	
Girth of Left Forearm—Contracted		24.97	25.32	25.67	
Girth of Right Thigh		49.76	50	50.77	
Girth of Right Calf		33.2	33.35	33.76	
Girth of Left Thigh		49.54	49.78	50.48	
Girth of Left Calf		33.14	33.33	33.7	
Depth of Chest—Repose		18.41	18.57	18.64	
Depth of Abdomen		17.3	17.32	17.4	
Depth of Head		19.35	19.33	19.42	
Breadth of Head		15.35	15.39	15.43	
Breadth of Shoulders		39.38	39.55	40.04	
Breadth of Chest—Repose		25.57	25.58	25.9	
Breadth of Waist		24.74	24.75	25.15	
Breadth of Hips		31.88	31.98	32.3	
Capacity of Lungs		395	401.2	406.4	
Strength of Right Forearm		45.4	47.54	50.84	
Strength of Left Forearm		40.85	43.22	45.2	
Strength of Back		131.75	138.95	144.0	
Strength of Legs		211.8	231.1	238.8	
Strength of Upper Arms (Push Up)		4.39	5.43	5.9	
Strength of Upper Arms (Pull Up)		5.46	6.22	7.0	
Total Strength		508.68	549.22	577.5	

The occurrence of flat foot and weak foot appears to be increasing from year to year, as shown in the following table.

ABNORMALITIES OF THE FEET

	Class of 1910, 364 Students Per Cent.	Class of 1911, 305 Students Per Cent.
Normal feet	61.2	48.2
Both feet flat	15.4	16.9
Both feet weak	20.4	29.5
One foot weak	1.8	3.6
One foot weak and one flat	0.9	1.7

The following table shows the occurrence of abnormalities of the spine. More than 95 per cent. of the abnormalities recorded in this table were postural and not pathological.

ABNORMALITIES OF THE SPINE

	Class of 1910, 365 Students Per Cent.	Class of 1911, 305 Students Per Cent.
Normal spine	18.3	15.4
Kyphosis (round back)	17.3	18.0
Lordosis (hollow back)	12.6	12.1
Scoliosis (lateral curvature)	6.6	4.0
All three abnormalities combined	15.6	13.7
Kyphosis and lordosis	20.5	29.0
Kyphosis and scoliosis	4.6	3.6
Lordosis and scoliosis	4.4	4.2
Scoliosis alone or combined	31.2	25.5

The nationality of the students is of interest in connection with the above data.

BIRTHPLACE OF 283 STUDENTS IN THE CLASS OF 1911

		Per Cent
United States	240	84.8
Russia	14	4.9
Canada	5	1.8
Austria	4	1.4
Roumania	3	1.0
West Indies	3	1.0
Italy	3	1.0
Hungary	3	1.0
England	2	
Scotland	1	
India	1	
China	1	
Germany	1	
South America	1	
Japan	1	

NATIONALITY OF PARENTS OF 288 STUDENTS

		Per. Cent
American (both parents)	100	34.7
German (both parents)	57	20.0
British (both parents)	33	11.5
Hebrew (both parents)	20	6.9
Russian and Pole (both parents)	16	5.5
Hungarian (both parents)	4	1.4
Italian (both parents)	3	1.0
Latin-American (both parents)	3	1.0
French (both parents)	2	
Scandinavian (both parents)	2	
Chinese (both parents)	1	
Japanese (both parents)	1	
Roumanian (both parents)	1	
Bohemian (both parents)	1	
American and foreign	12	4.1
Mixed foreign	19	6.6
Three or more nationalities represented in parents and grandparents	13	4.5

G. L. MEYLAN

THE AMERICAN FEDERATION OF TEACHERS OF THE MATHEMATICAL AND THE NATURAL SCIENCES

GENERAL STATEMENT OF THE POLICY

THE officers of the federation deem it advisable to make the following brief statement of plans and policy, for the information of associations which may be interested in joining the organization.

The general purpose of the federation is to increase the efficiency of the federated (local) associations by bringing each of them into helpful cooperative relations with others that are working along similar lines in other parts of the country, and with the new Section L on Education of the American Association for the Advancement of Science.

The work of the federation through its officers and committees is expected in the near future to be developed along the following principal lines:

As a clearing house for the federated societies the federation will undertake to collect and to keep up-to-date information in regard to the work and the publications of these societies, and to aid as opportunity offers in the formation of new societies when needed.

As a publishing agency the federation will systematically print such of this information as may be of general interest in simple bulletins or in reports in scientific periodicals. Each federated society will be furnished with a list of the principal papers published in its field, and available to its members by purchase or exchange.

As a cooperative organization the federation will from time to time, of its own initiative or at the instance of a particular society, propose questions of general interest for the consideration of the federated societies, or appoint committees on questions of national scope in the teaching of science.

In relations with national societies, such as, for example, the American Association for the Advancement of Science, the National Educational Association, the National Society for the Promotion of Industrial Education, etc., the federation will endeavor to secure due recognition of the interests of the associations composing it, and of the great body of teachers of science.

At its Chicago meeting the American Association for the Advancement of Science showed its interest in and approval of the movement by affiliating the new federation with itself, and then extending to members of the federated associations the opportunity of joining the association without the usual initiation fee.

In general, the federation is expected in the future development of its policy to promote the advancement and improvement of science teaching in whatever manner may seem wise under the restrictions fundamental to its organization, which leave entire freedom of action to the federated associations, and which contemplate the transaction of federation business mainly

by correspondence, and with limited funds.

In spite of these limitations, it is the opinion of the officers of the federation that it will fill an important need and render a valuable service. They confidently appeal on this basis, too, for the support of associations which have not yet already joined the federation, with the hope of beginning the work outlined at an early date. H. W. Tyler, *Chairman*,

Association of Mathematics Teachers in New England.

R. E. Dodge,

New York State Science Teachers' Association.

F. N. Peters,

Missouri Society of Teachers of Mathematics and Science.

J. T. Rorer,

Association of Teachers of Mathematics in the Middle States and Maryland.

C. R. Mann, *Secretary*,

Central Association of Science and Mathematics Teachers.

NEW ENGLAND FEDERATION OF NATURAL HISTORY SOCIETIES

The annual meeting of the New England Federation of Natural History Societies was held in Boston on Friday and Saturday, April 17 and 18. The result of the election was the following board of officers for the coming year: president, John Ritchie, Jr., Boston Scientific Society; vice-presidents, Arthur H. Norton, Portland (N. H.) Society, Norman S. Eaton, Fall River (N. H.) Society; secretary, J. H. Emerton, Cambridge Entomological Club, and treasurer, Miss Delia I. Griffin, Fairbanks Museum, St. Johnsbury, Vt. Two councillors for each of the New England states were named. The principal business of the federation was the arrangement of the details of the Mount Washington field meeting, July 1–8 of the present year. The federation will assemble at the Summit House on Wednesday evening, July 1, for its own meeting. The other evenings will be devoted to the meetings of the states, Friday being preempted by Vermont, at which time the regular summer joint-meeting of the Vermont Botanical Club and the Vermont Bird Club will be held. Features of the field-meeting will be rambles to the stations of the alpine flora and the geological exposures, trips into the ravines and along the ridges and evening meetings where papers illustrated by the lantern will be presented. It promises to be an important meeting, about twenty-five of the New England societies promising representatives. Members of the Appalachian Mountain Club which is to hold its regular field-meeting at the Profile House at the same time will be in attendance and it is proposed to make the summit the objective point for an excursion of Section E of the American Association for the Advancement of Science, which will begin its sessions on Monday, June 29, at Hanover. Mount Washington, therefore, promises to be the scene of active scientific gatherings during the early summer.

In connection with the annual meeting in Boston there was a two-days' exhibition of material and methods. Much of this had relation to the help given by local societies to the public systems of education. The Barton Chapter of the Agassiz Association, represented by Misses Cherrington and Clapp showed many specimens of mosses, Miss Cora H. Clarke, many specimens of galls, Mr. Emerton, his portable comparison collection of spiders and the Lawrence Society, of New Hampshire, a collection of local birds' nests. The Worcester Society, of New Hampshire, exhibited its bird sets for public school teachers, its moss specimens for its free classes in botany and some local collections. It showed also photographs of the Dodge collection of birds, a splendid special one of Worcester County, valued at ten thousand dollars, given to it by Mrs. E. H. Dodge, a gift that was supplemented by the works of Audubon, presented to the society by Thomas H. Dodge, one of the foremost of Worcester's wealthy citizens. Architects' drawings were also shown by the society of the new home that the society hopes to have, a building that will place the natural history interests of the city on the same high plane as those made possible for art by the recent Salisbury bequests.

GEOGRAPHICAL FIELD STUDY IN ITALY AND THE ALPS

IN connection with a summer trip abroad, Professor W. M. Davis, of Harvard University, proposes to spend June and the greater part of July in northern Italy and the Alps, studying certain geographical problems. He would be pleased to have associated with him a number of advanced students, already somewhat practised in field study. The districts proposed for examination and the general itinerary (subject to change) are as follows: The foothills of the Apennines near Ancona (about June 1); the non-glaciated valleys of the Apennines in the neighborhood of Faenza (about June 5); the basin of Florence (about June 10); the Mediterranean coast, between Pisa and Spezia, between Spezia and Genoa, west of Genoa (about June 15–20); the divide between Mediterranean streams and the headwaters of the Po, north of Genoa (about June 22); changes in river courses in the headwaters of the Po (about June 24); glaciated valleys and lake basins in the Alps (Como, Lugano, Maggiore) (about June 25–30); the non-lacustrine valleys of the Dora Baltea and Dora Riparia (about July 2–6); glaciated valleys of the French Alps near Grenoble (about July 8–15).

It is desired to treat the problems above outlined, and such other problems as members of the party may desire to take up, in accordance with the methods of systematic physiography. The method preferred involves the consideration of *structure, process and stage;* that is, each district is to be regarded as a solid mass, made up of certain geological structures (composition and attitude of rocks), standing at a certain altitude with respect to baselevel, and advanced by certain erosional processes to a certain stage of physiographic development. The possibilities and the limitations of verbal description are to be carefully considered.

It is not intended that the members of the party should travel continuously together. The party may be joined and left at any time, as agreed upon. It is requested that any person who desires to take part in these studies should send to address given below, at Cambridge or in Italy, a statement of his qualifications and (in case he does not hold a university appointment) a letter of introduction from the professor of geography with whom he has recently studied.

If his qualifications appear sufficient, word will be sent giving place and date of *rendezvous*. On meeting, some local problem such as above indicated will be taken up, its general nature will be set forth, the methods of treating it will be discussed, and two or three days will be spent in the field with such members of the party as are there gathered. Professor Davis will then go on to some other locality, leaving the party to continue local study of their problem for several days. At the end of that time they will overtake him at a new locality, where a report will be made on their first piece of work and (with such new members as there join the party) a plan for a second piece of work will be developed; and so on as long as desired, but not later than July 20.

Professor Davis does not desire to assume any responsibility as to arrangements of travel, hotels, trains, etc. Each member of the party is to make his own traveling arrangements and to pay his own traveling expenses. At the same time, while members of the party are together, it is expected that plans of travel will be made in common. A general reunion is proposed about July 18, in or near Grenoble, for the presentation and discussion of the various studies. The languages of the Geographical Congresses (French, German, Italian, Spanish or English) may be used. Professor Davis may be addressed as follows: care of W. J. Turner and Co., Naples, until May 10; care of Sebasti e Reali, Rome, until May 20; care of Kuster and Co., Turin, until June 30.

SCIENTIFIC POSITIONS IN THE PHILIPPINES

THE Bureau of Science of the Government of the Philippine Islands at Manila announces two vacancies in the chemical laboratory: one

at a salary of $2,000 per year for candidates with the doctor's degree, the other at $1,600 a year open to those with a bachelor's degree. The bureau desires candidates from the leading American universities where chemistry is well taught. The training should be the usual thorough one in the preparatory studies such as physics and mathematics, and in general inorganic, analytical and organic chemistry, together with a course in organic preparations. The research work for the doctor's degree may be either in inorganic or organic chemistry.

Candidates who are thoroughly equipped, who are accurate and painstaking in their work, who have the proper technique and capability for individual investigation, will have opportunities for promotion to vacancies occurring in positions above them. The salaries in the division of chemistry range from $1,600 United States currency per year, through intervals of $200 and $250, to $3,000 per year.

The laboratory is fully equipped with all modern apparatus and with a complete scientific library.

All information in regard to these positions can be obtained from the Bureau of Insular Affairs, Washington, D. C.

Two positions in the biological laboratory for men thoroughly trained in bacteriology and pathology are vacant in the Bureau of Science, Manila. One of these is at a salary of $2,500 United States currency per year. and it is desired if possible that the candidates should have had training in the principles and technique of serum preparation and therapy, and it is hoped that the successful one shall acquaint himself with the serum work carried on in the Bureau of Science and become capable of operating the serum section of the biological laboratory. The other position is for an assistant in bacteriology and pathology, and candidates must be capable of doing original work and must have the degree of doctor of medicine.

The opportunity for the study of tropical diseases is unsurpassed, and the candidates, through the diagnostic work in the hospital and in the government prison, will have every opportunity to study a great variety of tropical infections and parasites.

The outlook for promotion is good, as vacancies occur in upper positions in the laboratory. The salaries range from $1,600 to $5,000 United States currency. The bureau publishes the medical section of the *Philippine Journal of Science,* so that all research work can be promptly edited.

All information in regard to the positions can be furnished from the Bureau of Insular Affairs, Washington, D. C.

SCIENTIFIC NOTES AND NEWS

At the meeting of the National Academy of Science held in Washington on April 23, members were elected as follows: Edwin Brant Frost, director of the Yerkes Observatory, University of Chicago; William E. Storey, professor of mathematics, Clark University; Edward F. Nichols, professor of physics, Columbia University; W. F. Hillebrand, chemist in the U. S. Geological Survey; Wm. B. Clark, professor of geology, the Johns Hopkins University; Whitman Cross, geologist, U. S. Geological Survey; E. G. Conklin, professor of zoology, University of Pennsylvania, professor-elect of biology, Princeton University; Theobald Smith, professor of comparative pathology, Harvard Medical School; Simon Flexner, director of the Laboratories of the Rockefeller Institute for Medical Research. Foreign associates were elected as follows: Svante A. Arrhenius, director of the Division of Physical Chemistry of the Nobel Institute of the Academy of Sciences, Stockholm; Joseph Larmor, Lucasian Professor of Mathematics at Cambridge University; Ivan Petrovic Pavlov, Imperial Institute for Experimental Medicine, St. Petersburg; Hugo Ritter van Seeliger, professor of astronomy in the University of Munich, and Th. Barrois, professor of parasitology in the University of Lille.

At a meeting of the council of the Boston Society of Natural History, held on April 15, it was unanimously voted that the Walker grand honorary prize of one thousand dollars be awarded to Dr. Grove Karl Gilbert, of the United States Geological Survey. This

award is made once in five years under the terms of the will of the late William Johnson Walker, a benefactor of the society, "for such scientific investigation or discovery in natural history as the council may think deserving thereof; provided such investigation or discovery shall have first been made known and published in the United States of America." The previous recipients of the Walker grand prize have been: Alexander Agassiz, Joseph Leidy, James Hall, James D. Dana, Samuel H. Scudder and Joel A. Allen.

THE bill providing a pension of $125 monthly each to the widows of Drs. James Carroll and Jesse W. Lazear has passed the senate by a unanimous vote.

PROFESSOR E. A. SCHÄFER, of Edinburgh University, is giving this week the Herter lectures at the Johns Hopkins University, the subject being "The Pituitary Body." On April 23 he lectured on "Internal Secretions" at the George Washington University. The lecture was followed by a smoker given by the Medical Society of the University.

DR. M. P. RAVENEL, professor of bacteriology in the University of Wisconsin, has been appointed director of the State Hygienic Laboratory.

PROFESSOR BASHFORD DEAN, of Columbia University, has been elected a corresponding member of the Paris Museum of Natural History.

THE Right Hon. A. J. Balfour, F.R.S., has been elected a corresponding member of the French Academy of Moral and Political Sciences in succession to Lord Reay, who has been elected an associate.

M. MAURICE HAMY, of the Paris Observatory, succeeds the late Dr. Janssen as a member of the Paris Academy of Sciences.

DR. J. N. LANGLEY, professor of physiology at Cambridge, has been elected a foreign member of the Royal Danish Scientific Society.

THE Back bequest for 1908 has been awarded by the Royal Geographical Society to Lieutenant George Mulock, R. N., on account of the survey work which he did on the National Antarctic Expedition.

PROFESSOR A. L. KROEBER, of the University of California, has returned from an ethnological visit to the Mohave Indians of Arizona and California. His investigations continued previous studies of the mythology, rituals and music of the tribe. A survey of nearly three hundred shellmounds on the northern shores of San Francisco Bay has recently been completed by the department of anthropology of the university.

AT the request of the Smithsonian Institution, the Department of State has appointed Dr. George Grant MacCurdy, of Yale University, a delegate on the part of the United States to the sixteenth International Congress of Americanists, to be held at Vienna, September 9 to 14, 1908.

MR. HARLAN I. SMITH, of the department of anthropology of the American Museum of Natural History, will continue his archeological reconnoissance of Wyoming, begun in 1907. A trip across the northeastern part of the state, possibly reaching western Dakota and southern Montana, is planned for the coming field season. The work is to begin to locate fields for future detailed cooperative explorations in this portion of the country which lies near the center of a very great area, regarding the archeology of which there is scarcely any literature and from which there are very few specimens in museums or elsewhere readily available for study.

THE Swedish government has proposed a grant for a scientific expedition to Spitzbergen this coming summer for geological and geographical research. Professor Gerard de Geer, the rector of the University of Stockholm, will be the leader of the expedition.

DR. CHARLES R. VAN HISE, president of the University of Wisconsin, will give the address to the graduates of the Michigan College of Mines on May 1.

THE Hon. James Bryce, British ambassador to the United States, will give the baccalaureate address to the graduating class of the University of Wisconsin on June 14.

DR. EDGAR F. SMITH, vice-provost of the University of Pennsylvania and professor of

chemistry, will deliver the annual address at the one hundred and fifty-second commencement on June 17.

By the will of Lord Kelvin, Lady Kelvin is appointed sole executrix, and all his property is bequeathed to her. According to the inventory, the value of the property is over $800,000.

Professor K. G. Abel, head of the industrial chemical laboratory at Stuttgart, died on March 9, at the age of fifty-eight years.

Professor Franz von Leydig, the zoologist, died at Rothenburg on Monday, in his eighty-seventh year. Dr. von Leydig studied at the Universities of Würzburg and Munich, and after holding a professorship at Tübingen for eighteen years, he was appointed to a chair at Bonn University in 1875. He is well known for his researches in comparative histology and anatomy.

The U. S. Civil Service Commission announces an examination on May 14, 1908, to fill a vacancy in the position of physical chemist, in the laboratory of the United States Geological Survey, at $2,160 per annum. No merely scholastic or clerical tests will be given, and competitors will not be assembled for examination. On May 20, there will be an examination for preparator of fossils in the Geological Survey, at a salary of $25 a month.

Prince Roland Bonaparte has given the Paris Academy of Sciences 100,000 francs for scientific research.

An expedition is being equipped, under the auspices of the Russian ministry of marine, with the object of discovering a northeast passage between the Atlantic and Pacific Oceans.

The thirteenth general meeting of the American Electro-Chemical Society is being held this week in Albany, N. Y.

The Bridgeport Medical Association has joined the Bridgeport Scientific and Historical Society this season in a series of nine popular lectures, which have been well received.

The Lake Laboratory maintained by the Ohio State University announces the usual program for the coming summer, including courses in general zoology and botany, entomology, ornithology, experimental zoology, comparative anatomy, ecology, embryology, invertebrate morphology and ichthyology; also opportunities for research work and accommodations for investigators as in previous years. The staff will include beside the director, Professor E. L. Rice, of Ohio Wesleyan University, Professor Lynds Jones, of Oberlin College; Professor Charles Brookover, of Buchtel College; Professor M. E. Stickney, of Denison University, and W. B. Herms, at present fellow in zoology at Harvard University. The opportunities offered are excellent for fresh water, and special attention is given to the aquatic life of the locality. Opportunities for research work in this line are very favorable. Independent investigators are given the use of tables free of charge, but are expected to furnish their own microscopes and other apparatus. The locality is an excellent one for summer work, the laboratory being situated on the point separating Sandusky Bay and Lake Erie, with its frontage on a fine beach. For circulars or detailed information letters may be addressed to the director, Professor Herbert Osborn, Ohio State University, Columbus, Ohio.

The spring lectures to be delivered in the lecture hall of the museum building of the New York Botanical Garden, Bronx Park, on Saturday afternoons, at four o'clock, are as follows:

May 2—"A Botanical Expedition to Jamaica," by Dr. Arthur Hollick.

May 9—"Early-flowering Trees and Shrubs," by Dr. N. L. Britton.

May 16—"Plant Life of the Sea," by Dr. M. A. Howe.

May 23—"Ornamental Shrubs: their Selection and Arrangement," by Mr. George V. Nash.

May 30—"Plants that Feed on Insects," by Dr. C. Stuart Gager.

June 6—"Adulterants in Foods and Drugs and their Detection," by Dr. H. H. Rusby.

From the annual report of the Nantucket Maria Mitchell Association we learn that a fire-proof observatory is in process of con-

struction on the memorial grounds on the Island of Nantucket, a few feet from the house (the birthplace of Maria Mitchell) which the association has owned and cared for since 1902, having brought together there certain scientific collections, as well as books and other material formerly the property of Professor Mitchell. Her five-inch telescope (Alvan Clark, maker), donated to the association, is in the hands of Alvan Clark & Sons Corporation, Cambridge, Mass.; they are providing a new mount and will themselves place it in the memorial observatory in good order when the building is ready to receive it, about the first of June.

WE learn from the *Electrical World* that the Niagara Scenic Commission has reported to the Secretary of War that the Niagara, Lockport and Ontario Power Company should be made to restore the beauty of the river bank at the point where its transmission cables cross the Niagara gorge. The point is about four miles from the falls, and the trees and shrubs were cut, broken and injured by the work necessary in the erection of the towers and lines. The scar left on the face of the cliff is not so bad on the New York as on the Canadian side, where the scenic commission has no authority, but the commissioners of Victoria Park may cause the restoration of the bank on that side.

THE central committee of the International Congress on Tuberculosis has, as reported in the *Journal* of the American Medical Association, announced the offer of prizes of $1,000, besides gold and silver medals, each prize to be accompanied by diplomas or certificates of award, for each of the following: (1) For the best evidence of effective work in the prevention or relief of tuberculosis by any voluntary association since the last International Congress, in 1905. (2) For the best exhibit of an existing sanatorium for the treatment of curable cases of tuberculosis among the working classes. (3) For the best exhibit of a furnished house, for a family or group of families of the working class, designed in the interest of the crusade against tuberculosis. (4) For the best exhibit of a dispensary or kindred institution for the treatment of the tuberculous poor. (5) For the best exhibit of a hospital for the treatment of advanced pulmonary tuberculosis. Other prizes offered are following: (1) The Hodgkins Fund Prize of $1,500 is offered by the Smithsonian Institution for the best treatise that may be submitted on "The Relation of Atmospheric Air to Tuberculosis." (2) A gold medal and two silver medals for the best exhibits sent in by any states of the United States, illustrating effective organization for the restriction of tuberculosis. (3) A gold medal and two silver medals for the best exhibits sent in by any state or country (the United States excluded), illustrating effective organization for the restriction of tuberculosis. (4) A gold medal and two silver medals for each of the following exhibits: (*a*) For the best contribution to the pathologic exhibit. (*b*) For the best exhibit of laws and ordinances in force June 1, 1908, for the prevention of tuberculosis by any state of the United States. (*c*) For the best exhibit of laws and ordinances in force June 1, 1908, for the prevention of tuberculosis by any state or country (the United States excluded). (*d*) For the best exhibit of laws and ordinances in force June 1, 1908, for the prevention of tuberculosis by any municipality in the world. (*e*) For the society engaged in the crusade against tuberculosis having the largest membership in relation to population. (*f*) For the plans which have been proved best for raising money for the crusade against tuberculosis. (*g*) For the best exhibit of a passenger railway car in the interest of the crusade against tuberculosis. (*h*) For the best plans for employment of arrested cases of tuberculosis. (5) Prizes of two gold medals and three silver medals for the best exhibit of a workshop or factory in the interest of the crusade against tuberculosis. (6) Prizes for educational leaflets: A prize of $100, a gold medal, and two silver medals in each class, for the best educational leaflet submitted in each of the seven classes defined below: (*a*) for adults generally (not to exceed 1,000 words). (*b*) For teachers (not to exceed 2,000 words). (*c*) For mothers (not to exceed 1,000 words). (*d*) For indoor

workers (not to exceed 1,000 words). (*e*) For dairy farmers (not to exceed 1,000 words). (*f*) For school children in grammar school grades (not to exceed 500 words). (*g*) Pictorial booklet for school children in primary grades and for the nursery. Dr. Charles J. Hatfield, Philadelphia, is the chairman of the committee, and Dr. Thomas G. Ashton, Philadelphia, is the secretary.

The Friday evening lecture at the Royal Institution on March 27 was given by the Hon. R. J. Strutt, whose subject was "Radioactive Change in the Earth." Lord Rayleigh was in the chair. According to the report in the London *Times*, Mr. Strutt remarked that the mineral pitch-blende, the source of radium and other radioactive materials, was in England only found in Cornwall, in veins in the granite and slate. The question arose, How did it get there? The answer he proposed to adopt was that it was derived from the surrounding granite which refined examination showed to contain radium to the extent of one part in a million million. Minute though this proportion was, the total quantity of radium contained at this rate in the external crust of the earth, to a depth of 40 or 50 miles, was more than sufficient to account for the internal heat of the earth. Of the constituents of granite zircon was found to contain quite a large quantity of radium, and in microphotographs of granite discolorations could often be perceived round a zircon crystal. Radium being present in granite, it was natural to expect the presence of helium also; and in fact that gas could be found if looked for with sufficient care. It could also be found in other minerals, the radioactivity of which was not very conspicuous, and he showed a sparking tube filled with helium which had been obtained from about 2 pounds of quartz. Radium also was to be found in numbers of other minerals he had examined, and generally in sufficient quantities to explain the amount of helium they contained. One exception he had discovered was beryl, which contained no radium worth mentioning, but a very large quantity of helium. After discussing a possible explanation of this exception, the lecturer concluded by saying that as the production of helium was a question of time, the quantity found in rocks of different geological strata might provide us with a means of estimating how much time had lapsed since their deposition.

UNIVERSITY AND EDUCATIONAL NEWS

Mr. Henry Wilde, D.C.L., F.R.S., already a liberal benefactor of Oxford University, has given £4,000 to found a Lectureship in natural and comparative religion.

Mrs. Gordon and Miss Peters have given £4,000 to University College, Dundee, for the erection of a laboratory of electrical engineering, in memory of their late brother, Lord Dean of Guild Peters.

An agreement has been reached in the matter of affiliation of Cooper Medical College with Stanford University. The study of medicine must be pursued in San Francisco and the trusts left by Dr. Levi C. Lane are to be fulfilled.

The regents of the University of Wisconsin at their meeting on April 22 considered the question as to whether the efficiency of instruction might be increased by providing separate classes in subjects generally neglected by men and by women, respectively; but as the matter was one of general educational policy, the regents deferred action until the faculty has an opportunity to consider the question and to report the results of its investigation to the board. A committee of nine professors in the college of letters and science, with Dean E. A. Birge as chairman, is now considering the matter, but owing to the complexity of the subject will probably not be able to report for some time.

At the University of Wisconsin Professor Carl C. Thomas, now head of the department of marine engineering of Cornell University, has been chosen to the professorship of steam engineering made vacant by the death of Storm Bull.

Professor Fred'k F. Jones, dean of the College of Engineering and Mechanical Arts in the University of Minnesota, has been elected dean of the academic faculty of Yale University. Professor Jones graduated from Yale College in 1884 and has been connected with the University of Minnesota since 1885.